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OF THE
American Philosophical Society
Held at Philadelphia for Promoting Useful Knowledge

VOLUME 92, NUMBER 5 NOVEMBER 12, 1948

RESEARCH FRONTIERS IN HUMAN RELATIONS

Papers read at the
Midwinter Meeting of the Society
February 6–7, 1948

THE AMERICAN PHILOSOPHICAL SOCIETY
INDEPENDENCE SQUARE
PHILADELPHIA 6
1948

The *Proceedings* of the American Philosophical Society is published as an annual volume and ordinarily is issued in five numbers. An author desiring to submit a manuscript for publication should send it to the Editor, Luther P. Eisenhart, American Philosophical Society, 104 South Fifth Street, Philadelphia 6, Pa.

The annual subscription price is five dollars; combined with a subscription to the *Transactions* the price is ten dollars. Individual copies of the *Proceedings* are offered for sale. This issue is priced at one dollar.

LANCASTER PRESS, INC., LANCASTER, PA.

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LIMITING FACTORS IN THE DEVELOPMENT OF THE SOCIAL SCIENCES

DONALD YOUNG

General Director, Russell Sage Foundation

(Read February 6, 1948 in the Symposium on Research Frontiers in Human Relations)

THE task of the social sciences, most simply stated, is the advancement of knowledge of human relations. The goal is the best possible understanding of the problems of living together in the modern world. This objective will not be challenged by anyone concerned with the survival of civilized society.

That the social sciences already have made useful contributions to the understanding of problems of human relations is evidenced by the fact that the mounting consumer demand for their product has far outrun their capacity. Only a few years ago social scientists were largely restricted to earning their living as academicians with skeptical students and each other as their main audience, and with little prospect that their wares might be marketable in the world of affairs. Today the various arms of government are unable to obtain all the social scientists for whom they have provision in their budgets. The same is true of business, industry, and agriculture. Universities and research agencies are similarly shorthanded. The result is that crucial work is left undone or in the hands of unqualified personnel. If this condition long continues, the resulting failures and disappointments will greatly impede the development of the body of social science knowledge needed for the orderly growth of society.

Potential consumers of social science products are not alone in their dangerous impatience for research findings which can be applied in resolving human problems. Social scientists themselves, because they are also citizens with social consciences, find it difficult and sometimes impossible to stay within the narrow bounds of their infant disciplines. It is not easy for price economists to hold to their rigorous research or to confine their professional advice to judgments firmly based on established knowledge when their country faces the ills of inflation. So, political scientists may be understandably impatient with meticulous search for principles in political behavior when there are so many obvious aspects of national and local administration in need of immediate reform. Similarly, anthropologists tend to be in haste to apply what they have learned from preliterate societies to the modern world, psychologists are tempted to leap from careful experimentation to broad interpretation, and sociologists not infrequently make pronouncements on social issues based on personal preference rather than on research. This very human eagerness to supply the over-expanded market for research knowledge of social behavior, if unchecked, must inevitably shake confidence in the value of the scientific approach to problems of social relations.

Social scientists have been so engrossed in their various substantive specialties that they have paid slight attention to the basic factors limiting their accomplishments. University courses in the main are organized in terms of substantive fields; students have to be taught about criminal causation and penology, comparative governments, labor relations, public opinion and propaganda, and so on. Trustees and administrators who determine salaries and allocate research funds naturally can be more readily interested in tangible products of research than in improving the process whereby they are produced. Material rewards and prestige go to the man who delivers the goods rather than to his anonymous precursors who paved the way. The way for social scientists, however, has not been well enough paved to permit merely casual attention to the underlying factors which determine the degree of research success or failure. These factors for convenience may be examined with reference to (1) the workmen, (2) their tools, and (3) conditions of work.

It already has been suggested that good workmen in the social sciences are fewer than existing opportunities for their employment. The shortage is much worse if one considers society's needs for verified knowledge of its critical problems. One is tempted to illustrate and dramatize this point with statistics. The total memberships of the various social science professional societies are pitifully small, but even so they give a greatly exaggerated impression of numbers because many people other than qualified research personnel like to attend their meetings, read their journals, or

just belong. And there is no limiting membership requirement more selective than willingness to part annually with five or six dollars. Graduate students and Ph.D.'s in the social sciences may also be counted, but a cursory examination of their motivations for advanced study, their courses of training, and their work and publication records is enough to demonstrate that only a small proportion may be expected to contribute to social knowledge. It is possible to approach the problem from the demand side through study of employment trends, with the hope that predictions of future needs may be based on projections from the past. If such predictions are to be valid for anything beyond the most immediate future, an assumption of social stability or undeviating development contrary to experience is necessary. Although new data which would permit estimation of probable needs for social scientists over the next decade or so no doubt could be obtained if time and money were available, existing statistics are of little worth for this purpose.

Fortunately, there is no need to torture available quantitative data into telling their dubious story or to assemble expensive new statistics. Such efforts at best would merely elaborate the obvious. All of us associated even peripherally with activities dependent on knowledge of human relations know of work which is not being done because personnel is not available or which is being done badly by unqualified individuals. Furthermore, thinking men are aware of the increasing complexity of social interrelations and concerned about the almost limitless issues of international relations, economic organization, family life, national and local government, taxation and other problems of social adjustment. It is encouraging to note that the number of social science graduate students is now very much larger than it was before World War II, but there is no reason to believe that the saturation point or even the minimum number needed to supply the demand for well-trained research workers in any field of social science is being closely approached.

There is a common belief among social scientists that achievement in their several disciplines would be rapidly improved if only better human raw material could be drawn into the social fields. Elbridge Sibley, who has recently completed a study of the supply of social science personnel, has been led by the available evidence to the conclusion that the basic problem of social science personnel does not lie in any failure of the social disciplines to attract a gratifying share of the superior individuals who enter the scholarly, scientific, engineering, and other professions.[1] His analysis is convincing that the social sciences are drawing a reasonable proportion of the best students who choose careers in these fields.

Less gratifying, however, is Dr. Sibley's conclusion that while "at both the undergraduate and graduate levels impressively large numbers of high-ranking students are majoring in social disciplines," it is also true that "student bodies in these departments are diluted by large numbers of students whose scholastic performance is undistinguished."[2] The problem of improving the quality of social science personnel thus appears to be that of refusing to accept or promptly diverting inferior recruits and at the same time perfecting facilities for training those of adequate ability. Our alibi that we could train personnel for research if only we had student material comparable to that in natural science departments has been shaken. The responsibility for demonstration is placed squarely on our own shoulders and those of institutional administrators whose duty it is to provide the opportunity and facilities necessary for the proper training of new recruits to the ranks of social scientists.

With few exceptions—and down to very recent years without exception—graduate departments in social science in even the best American universities have made grossly inadequate provision for research training. The so-called dilution of the social science student body by a disproportionate number of unqualified individuals naturally has made rigorous methodological instruction practically impossible even at the graduate level. The unfit students have been too serious a drain on professorial time, and they have inevitably necessitated lowering standards of instruction and achievement. There is little hope for major improvement so long as this condition is allowed to persist.

The careful selection of smaller numbers of the best candidates for graduate instruction may seem to be the simple solution of this problem, but while such selection is both possible and necessary, it is not easy. University administrations and many professors themselves may be impressed by large enrollments and consequently reluctant to limit admissions and weed out the unfit. Humanitarian

[1] The problem of social science personnel: Recruitment or training? *Social Sci. Res. Council Items* 1 (4): 1–7, 1947.

[2] *Ibid.*, 7.

considerations also play a role in the acceptance of inferior students and in the common practice of allowing them to struggle along indefinitely after their unsuitability for advanced work has been demonstrated. It is disastrous as well as human to be proud of large enrollments and superficially kind to the weak.

More fundamental in the problem of selection is the lack of agreement or even concern about the requirements for success in research in the various social disciplines. There is obviously a correlation between the level of general intelligence and success in any professional career, and this factor can be taken into account if there is sincere desire to do so. Whether there are more specific innate abilities predisposing to success in any particular social science is still unknown and can hardly be taken into account in any present selective process. There is no reason, however, why more attention should not be given to undergraduate preparation and early graduate achievement in determining what individuals may begin and continue graduate research training.

From the limited view of those interested in the improvement of research training, it is unfortunate that undergraduate social science courses are universally organized and taught almost exclusively for purposes of general education or occupational preparation. Furthermore, there is little uniformity in either the pattern or individual content of social science courses in even the better American colleges and universities. There is consequently no easy way to select students for admission to graduate study in the social sciences on the basis of some standard of substantive and methodological preparation, nor on the basis of any accepted criteria of research aptitude and interest. The students themselves, since they have encountered undergraduate social science courses primarily aimed at citizenship and vocational training objectives, have had little chance to develop elementary research skills or to acquire appreciation of the research basis of the courses they attended. The significance of the nonscientific nature of undergraduate social science courses, in relation to both the student's choice of a research career and the problem of graduate selection, is emphasized by contrast with the relatively uniform scientific organization of natural science curricula. As one of those who have argued stubbornly that natural science courses need not all be so technically organized as they usually are, and that more attention could profitably be given to the development of physical and biological science courses with greater emphasis on general education objectives, I am convinced that the solution to the problem of undergraduate preparation for advanced work in the social sciences does not lie in revision of social science courses after the natural science pattern although more of the research point of view than is now customary could well be presented. It may lie primarily in the development of supplementary courses emphasizing the research approach to questions of human relations, for election by those who choose to prepare for research careers in social science.

Failure of undergraduate social science curricula to give an understanding of the nature and spirit of research is largely responsible for the fact that a great proportion of graduate students in social science are not motivated to specialize in research. Experience as a graduate instructor and acquaintance with many individuals who hold the Ph.D. in social science and with their publications convince me that only a few of those who call themselves social scientists have as their professional goal the contribution of some research increment to the understanding of social relations. Graduate study generally seems to be undertaken as preparation for teaching or for administrative work in government or business, as supplementary training for the ministry or other activity in the interest of human welfare, or as a step vaguely considered useful in advancing personal notions of social reform. In many students there is a confused mixture of several of these conflicting motivations. As long as such confusion persists in the minds of students enrolled in common courses of graduate study, there will be compromising procedures in instruction and consequent dilution in the quality of research training.

Handicaps to research training inherent in the existing body of graduate students are paralleled by shortcomings in university facilities. Too small a proportion of social science professors are primarily interested in or qualified for research; fewer still are those whose other commitments permit them to give proper attention to developing the talents of the best research students. Furthermore, academic administrators, considering their over-all budgets, understandably seem inclined to let expensive graduate work be a secondary concern. They may be influenced by the tradition that research in the social sciences is something of a luxury, a leisure-time activity carried on mainly in the library or on summer visits to ar-

chives. Undergraduate social science courses can be lucrative, and advantage is taken of this fact as is shown by the higher ratio of students to teachers in these fields in comparison with ratios for entire institutions. For example, in three great middlewestern universities in 1946 the ratios of students in social science to staff were 22 to 1, 35 to 1, and 50 to 1, while the comparable general ratios for the same institutions were 9 to 1, 19 to 1, and 30 to 1. Similar differential treatment in the allocation of university research funds can also be pointed out at almost any institution. The combination of faculty weakness and administrative discrimination, however accidental these circumstances may be, is a crucial handicap in the advancement of the social sciences.

At first thought it might seem that the research training problems of the social sciences should be met by bringing their graduate programs into some close approximation of the most successful pattern found in the natural sciences. This would be unwise, if only because it is clearly impractical. Existing social science patterns with their traditions, commitments, and vested interests cannot and need not be revolutionized; they can and must be modified so that they will better serve their functions.

The alternative to imitation of the natural sciences is the differentiation of graduate programs in accordance with varying student purposes and with existing and potential university capacities. The legitimacy of student demand for advanced instruction which affords training for careers as social practitioners in economic life, in government, in welfare activities and in other fields of social relations may not be questioned. The growing number of schools of business, of public administration, of social work, and so on, is effective recognition of the need for professional personnel trained to apply the findings of social science along with the results of experience in practical affairs. But at present few laymen and even few social scientists are mindful that the foci of motivation and skill essential in the development of practitioners who use the results of research differ from those essential in the training of research men whose function it is to advance scientific frontiers.

If pioneers among leading individuals and institutions in both the applied and research fields in social relations will give concrete recognition to the distinctive needs of the social practitioner and the social scientist, it should be entirely feasible to improve research training in their departments and universities. Considering the research problem alone, the essential modifications in existing graduate practices would be: (1) better selection of graduate students on the basis of general ability, research motivation, and undergraduate preparation, with at least minimal requirements adapted to the various fields of specialization; (2) graduate curricula especially designed for the selected research students, which would largely exclude courses diluted by students with nonresearch objectives; and (3) emphasis on learning research methodology by engaging in research in the social science equivalent of the laboratory, i. e., an operating research organization such as the Food Research Institute at Stanford, or a suitable research project under the direction of a competent man. There is nothing revolutionary in such a program; scattered individuals are being trained in accordance with its essential features at a number of universities today. Apart from budgetary provision to meet the cost, all that is needed is formal recognition of the principles involved and establishment of definite graduate school units under mandate to adhere rigorously to these principles.

Research personnel may hardly be considered apart from the research tools at their disposal. The tools now available to social scientists are certainly primitive in terms of tasks to be accomplished, but the progress made in adapting, sharpening, and replacing instruments in use at the turn of the century is encouraging. Wisdom and experience accumulated by others and through orderly personal observation not so long ago were the main dependence of social scientists—or should one call them social philosophers?—who offered no apologies for their method.

An outstanding analysis of the behavior of a selected group of people and certainly the most influential work in the development of both sociology and social psychology ever written in this country is *The Polish Peasant in Europe and America* by W. I. Thomas and Florian Znaniecki, originally published in five volumes in the years from 1918 to 1920. This study is so uniquely outstanding that it was the first of a small series chosen for analysis under the auspices of the Social Science Research Council in 1938 in an attempt to gain better understanding of methodology through the appraisal of the best in recent social science. The intensive analysis subsequently made by Herbert Blumer was in the main accepted without serious challenge by the authors them-

selves as well as by their professional colleagues. In a sense it is unfair to quote just one paragraph of Dr. Blumer's findings, but it may serve to illustrate the authors' ways of working:

> It seems quite clear that Thomas and Znaniecki did not derive all of their theoretical conceptions from the materials which are contained in their volumes or from similar materials which they did not put into their volumes. Perhaps not even the major theoretical conceptions were derived from them. Indeed, the major outlines are foreshadowed in the previous writings of Thomas. It is rather self-evident that the authors began their study of the Polish peasant with the rudiments of their primary theoretical schemes, built out of much experience with human beings, many reflections and observations on human conduct, and considerable appreciation of human nature. Only individuals with such experience and gifts could have made the stimulating and incisive interpretations that they have made. It is further self-evident that their *particular* interpretations of Polish peasant life were not formed solely from the materials they present; we have to assume that the familiarity with Polish peasant life which enabled their interpretations was made in a wide variety of ways. Thus, while there can be no question but that much of the theoretical conception of the authors came from handling the documents, it is also true that a large part of it did not.[3]

In justification of the acceptance of this work as scientifically significant Blumer's summary "listing of some of the more important contributions" it made to sociology and social psychology may be quoted:

> 1. A demonstration of the need of studying the subjective factor in social life.
> 2. The proposing of human documents as source material, particularly the life record, thus introducing what is known as the life history technique.
> 3. A statement of social theory which outlines the framework of a social psychology and the features of a sociology. The view of social psychology as the subjective aspect of culture has been particularly influential.
> 4. A statement of scientific method which has stimulated and reinforced the interest in making sociology of scientific discipline.
> 5. A number of important theories, such as that of personality, that of social control, that of disorganization, and that of the four wishes.
> 6. A variety of concepts which have gained wide acceptance, such as attitude, value, life organization, definition of situation, and the four wishes.
> 7. A rich content of insights, provocative generalizations, and shrewd observations.
> 8. An illuminating and telling characterization of the Polish peasant society.
>
> What is perhaps of chief importance is the marked stimulation which it has given to actual social research.[4]

The work of Thomas and Znaniecki is offered as a superior example of what can be done to advance knowledge of social relations with an approach and tools now suspect. Possibly too many individuals working in the social fields today still depend on wisdom and crudely recorded experience without recourse to sharper research tools now available, and with vastly more confidence in their wisdom than is justified. On the other hand, many more wise men of living experience are needed in the social sciences not only while these disciplines remain in their current pioneer stage but also as long as their frontiers have any hope of extension.

A second approach which characterized early social science research and is still common may best be described as analogous to a fishing expedition with whatever tackle and in whatever waters happen to be handy. This approach begins with a decision that it would be a good idea to study some event, group of people or mass of data. For example, the objective may be to study the social consequences of a hurricane or other disaster, the influence of the Tennessee Valley Authority on the lives of the people in its area, criminals in a penitentiary, the inhabitants of San Francisco, the archives of the Office of Price Administration, or census data on housing. The techniques range from the undiscriminating collection without benefit of scientific criteria, of what seems to be interesting, through a kind of verbal photography, to intricate manipulation of quantities of statistics. The hope is that something subject to generalization may turn up, and there is naive confidence in the intrinsic merit of journalistic description or of statistical tables and charts. Hypotheses, except of the broad, vague variety ill-adapted for investigation with the materials and tools available, are absent.

Such fishing expeditions should not be confused with the collection and ordering of data which may be useful for practical purposes, interesting in the story they tell about a specific incident or situ-

[3] Blumer, Herbert, Critiques of research in the social sciences: I, An appraisal of Thomas and Znaniecki's The Polish Peasant in Europe and America, *Social Sci. Res. Council Bulletin* **44**: 74, 1939.

[4] *Ibid.*, 81 f.

ation even though not suitable for generalization, or possibly of value in later research studies more sharply focused. Explorations of the latter type can be helpful in deciding on the probable fruitfulness of possible research projects and in planning their design. The distinction between fishing expeditions and justifiable surveys lies in the unfounded scientific pretense of the former in contrast with the limited and specific practical or exploratory objective of the latter.

Leaders in social science today are increasingly emphasizing the necessity for concentrating in large measure on a third approach, namely, use of research techniques which will give results incontrovertibly convincing to their colleagues and to a wider public. As a necessary condition, they insist on the careful framing of research questions which at least have a chance of being answered with the data and tools at hand. They want results to have applicability beyond the case under investigation and to have predictive value. With this purpose statistical techniques are being applied to problems in all social disciplines, and the utility of the case method is being expanded. As both methods are improved, historical as well as contemporary materials are revealing more significant relationships. The appreciable measure of success which has attended this increasing concentration on making social science more scientific gives certain promise that further effort in this direction will be rewarding.

Social scientists, nevertheless, are still limited by the inadequacy of their research tools, and this situation is likely to be improved only with frustrating slowness. The pressure is stronger to add to knowledge about the family, race relations, the economic structure or political organization by techniques already known, however inadequate—than it is to work on methodological refinements. Concepts and integrated theory compatible with existing knowledge and essential for orderly analysis are far from adequate for the needs of social research, yet many social scientists tend to be no better than irritatingly tolerant toward colleagues who pay much attention to their development. It is axiomatic that scientific knowledge is cumulative, that well-designed projects must be capable of repetition, that procedures must be convincing at least to fellow scientists. Yet only a very few research projects are designed to build directly on the results of previous studies; in most social fields few projects are capable of repetition and fewer still are ever repeated; and plausibility is almost as readily accepted as methodological integrity. We may be encouraged, however, by the growing recognition of the importance of overcoming this limitation.

The quality of research depends on conditions of work as well as on the abilities and skills of research personnel and the tools they use. Critics of the social sciences have been wont to say that the way to improve these conditions is by improving the social sciences. This is a superficial comment; obviously, research accomplishment and the milieu in which it is done are interdependent but the interrelationship is not so easily analyzed.

Money adequate for efficient operation is the condition of work which inevitably comes first to mind when factors limiting research are under discussion. It is undoubtedly true that much more money is available to social scientists than can possibly be spent efficiently for research though this judgment may seem startling. The apparent shortage of research funds for social scientists reflects ineffective allocation rather than the sum actually and potentially available. In general, neither private donors nor government officials with funds for research seem confident that they can find competent research leaders and trust them with the essentially professional task of selecting projects and personnel. Designation of areas of social research which they wish to support is of course a reasonable prerogative of donors and public officials—but, with very few exceptions, they also decide on the relative technical merits of projects and procedures, and on the relative qualifications of professional personnel.

The limitations at the sources of funds constitute the greatest financial handicap for social scientists. In spite of congressional antagonism to the proposed inclusion of the social sciences in legislation to establish a national science foundation, liberal federal funds are available to social scientists through both civilian and military agencies. The difficulty is that, with a few exceptions such as the Department of Agriculture, these agencies have had little experience with social science and consequently they are vague about its potentialities in relation to their needs, unacquainted with the qualifications of personnel, and inept in the selection and design of projects. Time undoubtedly will do much to correct this situation, but meanwhile in the minds of many the mistakes of the agencies will be charged against social science.

Philanthropic foundations are a traditional and

gratefully regarded source of funds for pioneer research in the social sciences. There is record of 23 American foundations in 1915; by 1930 this number had increased to 151. Tax legislation may be given the major credit for increasing the number into the thousands today, if the definition of the Treasury Department is accepted. But before becoming too optimistic about this growth of philanthropic agencies it will be well to consider a summary statement of their nature by Raymond Rich and William Cherin Associates:

> Only a small proportion of the recent wave of foundations are as broadly conceived or as richly endowed as the great pioneers in the field. Many of them are, in fact, committed by their charters to the support of specific institutions or highly particularized purposes. A large number of others have gone to the other extreme in that they have taken as their purpose the broad but relatively uninformative wording of the tax-exemption provision of the Federal Revenue Act of 1934, Section 101—"for religious, charitable, scientific, literary, or educational purposes."
>
> Even more disturbing, however, is the fact that a great number of these new foundations have apparently thus far chosen to be known only to God and the Collector of Internal Revenue. This reticence is highly regrettable since it easily leads to the assumption that there must be something improper about them, and tends to cast a shadow on the foundation movement as a whole.[5]

By definition, foundations are private agencies for the distribution of wealth for the public good, but from the donor's point of view they may also be regarded as defenses against high income and estate taxes or as instruments for the enhancement of prestige or other personal purposes. Since foundations stem from such mixed motives, and are often dominated by the donor and operated by trustees and staff without research understanding, training, or appreciation, it is natural that only a few have thought of social welfare—usually their ostensible purpose—as being closely related to social science, and that even a smaller number have been financially responsible for significant contributions to research in human relations. Providers of funds and other laymen have learned that health depends on medical research controlled by medical men. It is well accepted that the physical and biological worlds yield most to man's needs and desires when attacked by natural scientists in accordance with their own strategies and tactics. Only small beginnings of a similar attitude toward the resolution of problems of human relations can be found in the foundations today, to say nothing of the public at large.

Industry, business, and agriculture are making significant and increasing use of all the social sciences in varying degrees, but primarily through applied studies and through adaptation of already accumulated findings to their problems. This is all to the good, for it gives recognition if not direct support to the practical value of social research. But in effect, it means that private enterprise is being indirectly subsidized. Such subsidy is common and generally accepted as proper, but there is, nevertheless, at least a moral obligation to contribute to the flow of research which is being tapped. Furthermore, serious harm is done research when personnel trained for scientific work at great expense is moved into private employment for nonscientific purposes. Leading businesses and industries already have recognized that it is to their own advantage as well as that of the public to help support both the training of natural science personnel through fellowships and also basic research in the natural sciences at academic institutions and in their own establishments. A few similar allocations have been made in social fields, such as the appreciable grants for work in industrial relations at Princeton, Chicago, Harvard, and elsewhere, and it seems certain that such aid will increase rapidly. It is less likely that there soon will be any great development of basic research within private enterprises themselves, and this is perhaps no cause for criticism in view of the shortage of research personnel and the relatively immature stage of research in most fields.

Most social scientists engaged in basic research are attached to universities and colleges, and in the last analysis it is the function of these institutions to see that necessary research funds are available to their faculties. Academic tradition, however, is not altogether favorable to the effective performance of this function. Whereas the natural scientists are strongly supported by tradition in getting funds for their research, the social sciences have only recently become insistent that their work

[5] *American foundations and their fields, Sixth Survey, Part I:* 5, N. Y., Raymond Rich and William Cherin Associates, 1947. The figures for foundation growth given above are also taken from this volume, pp. 4 f.

For a brief review of the foundation movement, descriptive statements of the organization of leading American foundations, and a bibliography on the subject, see Shelby M. Harrison and F. Emerson Andrews, *American foundations for social welfare,* N. Y., Russell Sage Foundation, 1946.

is also a proper charge against the budget. Few intelligent academic administrators would care to argue that members of the department of chemistry should not practically all be engaged in research and that the quality of the department should not be judged by their scientific contributions. In the social sciences teaching and administrative accomplishments are commonly accepted as at least on a par with research, and administrative discrimination between publications which gives greater weight to scientific articles than to learned essays, descriptive reports, syntheses in the nature of text books, or even popularizations is rare. This situation is being steadily corrected, but it is still an unfortunate condition of work for most social scientists.

The need for rational revision of the processes whereby money is allocated to the social sciences so that a more reasonable proportion will be expended for basic work is no more crucial than the need for rational organization for research so that social scientists may have adequate facilities for meeting their opportunities and responsibilities.

In considering the Social Science Research Council's concern with problems of research organization, there has been emphasis on the fact that,

> organization for research in the social sciences does not imply skepticism concerning anticipated contributions from lone workers nor does it suggest a future reduction in the research role of the individual investigator. It is traditional in the United States that research is an individual concern, and that the primary purpose of research organization should be to supplement rather than supplant individual effort. It is assumed with good historical reason that basic innovations in social science rarely emerge in the course of the extensive operations of large-scale projects; usually initiation of such projects has been stimulated by the product of some previous innovator working on a modest scale. It is to the lone worker that we shall continue to look for new ideas and pioneering studies; even much of the routine spadework of verifying the findings of others and filling in lacunae in our knowledge must be left in his care. It is a major function of research organization to make it possible for the individual to carry out effectively the task for which he is best equipped.[6]

Some indication of the variety of research organization devices adopted by universities is found in the names which have been given them. They are known as committees, councils, institutes, centers, bureaus, departments, divisions, laboratories, foundations, commissions, stations, schools, sections, boards, projects, and offices.[7] Any attempt to understand the functions of a university research agency from the term which occurs in its title must fail. There is no logical reason, for example, why labor and industrial relations research should be conducted by a "section" at Princeton University, a "department" at the University of Pennsylvania, a "division" at Stanford University, a "center" at Yale University, a "board" at Ohio State University, and an "institute" at the University of North Carolina. The confusion in terminology, unimportant in itself, reflects a bewildering diversity of function and procedure.

Some university research agencies restrict themselves largely or exclusively to aiding the individual projects of faculty members by means of small cash grants and nothing more. A second type of university research organization is concerned with the planning and conducting of a program restricted to a selected area of investigation, as in the case of the Office of Population Research at Princeton University. A third category of such agencies comprises those engaged in research planned, if not always initiated, under their own auspices and conducted under their supervision, but not confined to one selected area of investigation. An example of this type is the Institute for Research in Social Science at the University of North Carolina. These categories are not mutually exclusive and the distinction may be merely one of focus. There are nearly two hundred university agencies of these kinds more or less actively in operation today, and very little logic is discernible in the variety of their administrative systems, financial structure, coverage, relations with parent institutions, and standards of performance.

If there is less bewildering diversity in private social science research institutions which do not have formal university affiliation, it is probably because there are fewer of them. They face all the problems confronting university research agencies except those growing directly out of their formal affiliations, and one more: This distinctive problem is the maintenance of breadth of vision, flexi-

[6] Young, Donald, and Paul Webbink, Current problems of Council concern in research organization, *Social Sci. Res. Council Items* 1 (3) : 1–2, 1947.

[7] See, for examples, Committee on Organization for Research in the Social Sciences, *Tentative directory of university social science research organizations,* N. Y. Social Sci. Res. Council, 1947, mimeographed.

bility of staff, and a spirit of alertness to new problems and methods which comes most easily from the stimulation and criticism of a faculty ranging over the entire field of knowledge. The main advantage claimed for the completely independent research agency is that its resources can be devoted exclusively to its chosen program. There need be no concern with teaching, university administration or other matters extraneous to research. In actual practice, however, the situation is not as ideal for research as might be assumed. Community and other service functions can be as distracting as teaching. Autonomy brings with it added problems of administrative routine. Funds must still be raised, and without the aid of university prestige and associations. Academic colleagues are a source of supplementary knowledge, ideas, encouragement, and constructive criticism. Students, undergraduate as well as advanced, may be not only a distraction but also a stimulating and balancing influence. Stability may be promoted, at no serious cost to research independence, by affiliation with a mature educational institution. There is no substantial evidence that research in the social sciences dissociated from academic life is more favorably situated for efficient operation by virtue of its isolation. Indeed, there seems to be nothing that an independent agency can do which cannot be done at least as well and possibly better in close association with an educational institution.

Although research leadership in the social sciences seems clearly destined to remain primarily a university function, the independent and semi-independent agencies undoubtedly will continue to play an important role in the future as in the past. Their variety is no less wide than that of university agencies. A few foundations, such as the Russell Sage Foundation and the Twentieth Century Fund, have engaged directly in social studies. Independent economic research institutions of importance, including the National Bureau of Economic Research and the Brookings Institution, can be counted on the fingers of the hand. Museums traditionally engage in anthropological research. Historical organizations, commonly in the form of state and local societies, are of real importance in that field. Political science has its independent agencies, such as the various bureaus of municipal research and the Chicago cluster of public administration organizations, although their primary function usually is of a service nature in which research as such is incidental. No comparable agencies are in operation within the fields of sociology and social psychology but this is understandable because they do not have parallel social institutions, as economics and government do, and they lack the colorful appeal of the preliterate peoples of anthropology and the traditional interest of history. As a rough generalization it may be ventured that the quality and intellectual influence of the work of these agencies in the United States are in direct relation to the degree of effectiveness of their communication and collaboration with at least one educational institution.

The critic of existing organization for research in the social sciences would be justified in suggesting that social scientists have not applied their knowledge of human relations and institutional administration to their own organizational problems. He might go further and point out that they have not even used their research techniques to discover why research organization has been allowed to grow in such disorderly fashion, nor to identify the elements of success and failure in their own systems of operation. There is no answer to such criticism except the unsatisfactory one that other interests have claimed a higher priority.

A third unfavorable condition of work is public misunderstanding of the nature, accomplishments, and potentialities of the social sciences as practical means for gaining knowledge useful in meeting problems of community, national, and world relations. The essence of this misunderstanding, ranging from indifference and skepticism to open hostility, may be found in the reports of the hearings on the Kilgore-Magnuson science legislation bill and in the subsequent congressional debates. While there was much encouraging support of the social sciences by natural scientists and members of the lay public, social research was also identified with the race equality movement, economic radicalism, the New Deal, dangerous reforms in general, and rampant "social society consciousness," whatever that may mean. The subject is threadbare and need not be further illustrated. Honest skepticism, however, when coupled with willingness to examine the record, is entirely justifiable and is shared by leading social scientists themselves. The only way to reduce this limitation of public misunderstanding is through wider dissemination of the story of the nature of social science, its results and its ambitions. This is something which social scientists, except for a few gifted individuals, cannot and should not do. Research will be most rapidly advanced if they will confine themselves to their proper tasks and

write their esoteric research reports for each other. What is needed is a special group of writers who can aptly translate research findings into language meaningful to the public and to persons responsible for their practical application.

A fourth and final limiting condition of work is the confusion between the functions of social research and social engineering which is disadvantageous to both. As has been suggested, graduate training in the social sciences rarely distinguishes between students preparing for research careers and those looking forward to careers in teaching or other occupations where knowledge of a social science may be useful. Social scientists themselves too frequently accept without question a dual role as scientist and practitioner without full awareness of the distinction between the two, although there is little reason to assume that a man well trained and skillful in the one profession necessarily will be adept in the other.

As Pendleton Herring has put it,

> One of the marked differences between the scientists working with inert matter and lower organisms and the scientists concerned with human functions and behavior is that the former have developed technicians trained for the specific job of putting results to practical use. These technicians include doctors, dentists, engineers, soil conservationists, etc. Their clearest counterparts in the social sciences are social workers, clinical psychologists, and graduates of schools of business and public administration. One of the greatest needs in the social sciences is for the development of skilled practitioners who can use social data for the cure of social ills as doctors use scientific data to cure bodily ills. At the moment the market can absorb many more trained social technicians than the universities are prepared to furnish.
>
> The term *technician* is a loose one, presumably applicable to anyone who uses "techniques," of whatever sort, and it would be difficult to deny the applicability of this term to congressmen, teachers, mechanics, or mothers. But the term *social science technician* should mean something fairly specific. It should mean an individual who has been professionally trained to apply to practical situations the facts, generalizations, principles, rules, laws, or formulae uncovered by social science research. . . .
>
> Social engineering might be defined as the application of knowledge of social phenomena to specific problems. Enough has been accomplished in these terms to warrant confidence that much more can be achieved in the future. For example, the work done in clinical psychology, in city planning, in public opinion polling, and in criminology indicates clearly that social engineering is a meaningful conception, worthy of considerable expansion.[8]

Until such time as social scientists, as well as others, recognize automatically that the research man and the practitioner are not interchangeable units, their research will be hampered both by diversion of their time and energy and by confusion of objectives. This statement should not be interpreted to mean that research and practice should be completely divorced; each can and must profit from the other, at times the functions will overlap, but only the rare individual can be truly skillful in the performance of both.

Thus far in this paper no reference has been made to the argument that the fundamental factor limiting social scientists in the accomplishment of their task may be the impossibility of applying scientific method to social data. From the social scientists' point of view this is a dead issue. The indefensible nature of such a theoretical position has been cogently stated in three recent papers published in *Science*. Marshall H. Stone in his paper on "Science and Statecraft" effectively challenged the common allegation that "human affairs are essentially unpredictable and beyond the reach of experiment, thus falling outside the scope of scientific inquiry" and the tendency on the part of some critics towards "characterizing all existing techniques as inadequate and simultaneously rejecting all innovations as speculative or unsound."[9] E. U. Condon in a paper on "Science and the National Welfare" stresses the need for the development of research in what he designates as the "humane" sciences. His faith in the predictability of human behavior is unequivocal:

> It is often said that man's social irrationality is a consequence of the fact that economics, sociology, and political science are not sciences but merely individual judgments and personal opinions. Now this is palpably untrue even at present, for much is known about cause and effect in these fields, and such statements are made only because habit, custom, tradition, and heritage tend to make us cling to whatever we know rather than to re-examine the data, coolly and critically.[10]

[8] Herring, Pendleton, The social sciences in modern society, *Social Sci. Res. Council Items* **1** (1): 5–6, 1947.

[9] Stone, Marshall H., Science and statecraft, *Science* **105**: 509, 1947.

[10] Condon, E. U., Science and national welfare, *Science* **107**: 6, 1948. *Cf.* Chapin, F. Stuart, Social obstacles to the acceptance of existing *social science* knowledge, *Social Forces* **26**: 7–12, October 1947.

James B. Conant, in similar vein, states that it is his

> belief that methods have already been developed to a point where studies of society by competent scholars can provide basic information to assist all those practical men who struggle with the group of problems we list under the head of human relations. Both fundamental investigations as to the nature of man and society and immediate studies of specific problems are required.[11]

The steadily increasing acceptance of the belief, particularly on the part of natural scientists, that the scientific approach is applicable to problems of human relations has been a most heartening development in the conditions under which social scientists are working. With such support from natural science colleagues the position of social scientists that they need not be too concerned about the outcome of the argument as it still persists is understandable. The undeviating trend is in keeping with the spirit and program of the American Philosophical Society and gives confidence to those of us in the newer disciplines that the concept of unity in science and learning is not an idle phrase.

[11] Conant, James B., The role of science in our unique society, *Science* **107**, 80, 1948.

STUDYING THE ATTITUDES OF SOLDIERS

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(*Read February 6, 1948 in the Symposium on Research Frontiers in Human Relations*)

THE Research Branch of the Information and Education Division in the War Department, in which some of us served during the War, provided opportunity to apply research tools of social science to aid social policy. Therefore, I have been asked to use this experience as the background for some *obiter dicta* on the relationship of social science to social engineering.

In a way, this is a bit embarrassing. For over two years since the War the promise has been pending that a series of volumes on which some of us have been working, based on this War experience, would be laid before the judgment table of our peers. The volumes are now nearly finished, and I would prefer to postpone further public discussion until others have had a chance for detailed, critical examination of our report. Social science has many more programs and promises of performance than it has evidences of performance. The most useful contributions to methodology are likely to be performances, with all their shortcomings, rather than abstract generalities about research.

However, there has been a good deal of confusion between social science and social engineering, or, as it is sometimes phrased, between basic research and applied research in social science. Today's meeting on Opinion Studies and Public Policy is held under such distinguished auspices that it does evoke a special obligation to attempt a contribution, based on war-time experience, toward clarification of some of the issues.

A phrase like "application of social science in a practical situation" implies that there is a science to apply. It implies the existence of a body of tested theory from which deductions can be made, applicable to new concrete situations.

In all candor, I do not think that social science, or at least those regions of social science with which I have most familiarity, does as yet have a body of tested theory from which many deductions can be made, applicable to new concrete situations. Hence, it is a fair question whether so-called social engineering of the kind, for example, we did in the War Department, can properly be called an application of social science.

In studying social attitudes, which were the main subject of our investigation, we were the beneficiaries, it is true, of a rich cultural heritage. At least three lines of influence have combined to offer a helpful conceptual frame of reference for such studies. One is from what is sometimes called dynamic psychology. The hypotheses and the data of psychoanalysis have worked something of a revolution in thinking in social psychology in one generation. Another is from what might be called learning theory, which has been important not only for its concepts but also for its examples of verification by the use of controlled experiments. A third is from social anthropology and sociology, the former in emphasizing the role of culture in shaping the personality and attitudes of the plastic individual and the latter in calling attention to such phenomena as the personal tensions which arise when an individual, being a member of several social groups, feels the strains for conformity to group values which are often conflicting, or to such phenomena as the variations in attitude associated with social status and social mobility.

Such theoretical contributions as these provide a useful orientation. But generally they are not as yet formulated into a body of propositions, rigorously tested in crucial instances, which enable one to predict in a new situation, that, given x_1, x_2 and x_3 in certain combination, then we will expect y.

To illustrate, let us consider a concrete problem. Should the Army try to make soldiers hate the enemy? It was held that if sufficient hatred of the enemy could be built up, some of the aggression then directed inward against the Army by frustrated soldiers could be redirected by displacement on the enemy. A number of very able social psychologists and psychiatrists sought to analyze this problem logically, making deductions from analytic theory and learning theory, and not only ended in disagreement but were not

particularly successful in stating clearly the crucial points at which basic research was needed before one could determine which points of view were more probable. Equally puzzling was the problem, "Could the Army make the soldiers hate the enemy if it tried to?" Surveys of combat troops showed, quite convincingly, that the closer soldiers got to the enemy the less they manifested evidences of hatred, except for momentary flare-ups of anger. Again there was much disagreement as to what the Army could achieve, although the principle was developed that an information policy which goes counter to the beliefs of the best informed was most likely to be regarded as "propaganda" and to boomerang. Evidence from surveys and from some experimental studies gave at least plausible support, if not demonstration, to this principle or rule.

The truth is that, except for providing broad and none too explicit ways of looking at problems, most of the theories of social psychology and sociology were not in very good shape for practical application. As a substitute, one had to fall back on empirical generalizations from common sense experience, and these, when dressed up in the jargon of our profession, become quickly confused with propositions deducible from scientific theory. The prediction, "If you hold your hand in the fire you will burn it," is verifiable, but it is hardly a law of physics, chemistry, or psychology. Most of the precepts which we use in training our children or those which sell books like Dale Carnegie's are about at this level. And, quite candidly, it is precepts like these, based, if you will, on enlightened common sense, upon which most of our social engineering falls back. It takes only a few demonstrations, like the classic examples of post-hypnotic suggestion, which dealt a body blow to common sense ideas about rationality, to indicate how wrong so-called common sense sometimes can be.

When we are using common sense and not explicit scientific theory, it is highly important that we be aware of what we are doing and especially that we do not wrap up what we say in a fancy package with a lot of polysyllables which eventually can beguile us as they beguile others.

In the general, in studying the attitudes of soldiers, we in the War Department did not often draw explicitly on rigorously tested scientific theory and, though aided in general outlook by broad points of view in our social science cultural heritage, we had to rely mainly on *ad hoc* applications of common sense experience. From the conceptual standpoint, then, our operation could hardly be called an application of social science to public policy.

But even when social engineering does not often apply specific theories of social science, it can owe a great debt to social science by using technical research tools developed by social scientists.

Here, again, we must be careful not to exaggerate what we owe to social scientists as such and what we owe to others, such as mathematicians, on the one hand, and practitioners, like newspapermen and practical opinion research men, on the other.

Actually, the techniques of research largely used in our studies of nearly half a million soldiers throughout the world, were not particularly sophisticated and were not techniques primarily developed by academicians. In formulating a problem by preliminary informal interviewing and in designing questions to be asked formally and systematically, we used the same devices as are used by any good newspaper reporter, though it would be more impressive if we called them by some such name as a clinical diagnostic interviewing technique, or anthropological field interviewing technique. In preparing and administering formal questionnaires, we relied primarily on the experience of public opinion and market research practitioners, with some modifications to suit the military environment. Some studies, it is true, involved experimental designs which only technicians skilled in the construction of controlled experiments could be depended on to carry through satisfactorily, but most of the operations did not call for skill superior to that found in the best public opinion research organizations. In sampling design and in satistical analysis rather specialized skills were demanded. The combination of good training in social science plus good training in statistics plus good common sense was exceedingly important at the analytical stage.

Alongside the practical work went a limited, though continuous, endeavor to improve the techniques. The inadequacies of conventional public opinion research are quite glaring and a serious effort was made to develop better techniques. Most of the staff had to run to fires, hence only a few could be spared to stay home and tinker with new and better fire extinguishers. It is my conviction that the main reason why our area of social science is still largely a conglomeration of broad and plausible theories and empirical gener-

alizations from common sense not too closely related to these theories, is that the formulation of hypotheses in the form, "Given x_1, x_2, x_3, then y," requires isolation and measurement of basic variables, and that this job has not been done. In the study of human abilities, the mathematical models of factor analysis are leading to the isolation and measurement of basic variables. Instead of talking about intelligence, we now talk about mental abilities (plural), and having tentatively isolated some of them, psychologists are preparing for the next step of finding under what conditions and to what extent these abilities are or are not modified by training. The implications for education and vocational guidance are tremendous. Similarly, in the study of attitudes, it is likely that conceptual progress depends on reduction of an almost endless variety of attitude responses to a more limited number and on manipulation of these basic variables when identified. The road to this goal is probably very long, and one of the most important contributions of our work in the War Department may have been to move a few stumbling steps along that road. Our mathematical work on scale analysis, which stood on the shoulders of previous work by psychologists and mathematical statisticians, was not so important in direct practical application as was our use of crude conventional techniques. But it did serve to keep our technicians aware of the limitations of the tools they were using in practice. And it has stimulated a new post-war attack on the problems of socio-psychological measurement which promises to become one of the crucial developments in our area of social science.

To summarize then, our operation benefited broadly from a cultural heritage deriving from dynamic psychology, learning theory, and social anthropology and sociology, but depended mainly on the use of empirical generalizations from common sense and on techniques used by other practitioners, many of which were not academic in origin or development. As a by-product, some contributions were made to social science techniques, especially in the demonstration of the utility of controlled experiments and in the attack on fundamental problems of measurement. Though most of us were social scientists, it is possibly more by courtesy than by fact that our operation could be called an application of social science to public policy.

That our operations did have some effect on Army policy is clear in the record. As time passes, there seems to be a human tendency for people associated in the war effort to imagine their contributions to be more far-reaching than was actually the case. So much of what went on is not on paper that future historians can reconstruct only scattered fragments and it would be hard to prove or disprove assertions as to the role played by any small unit in the global struggle. It is all the more necessary, then, that we who had something to do in connection with the War be cautious in our claims.

The present paper can perhaps be most helpful if we merely attempt to illustrate one condition which seemed to be required if our research was to be useful to policy.

That condition was direct access to the policy makers, well in advance of policy making. This was particularly difficult in the Army, whose insistence on "channels" perhaps differed in degree but not in kind from that in other bureaucratic enterprises in government or business.

Let us illustrate first with a relative failure. Our earliest studies, the first of which was made the day after Pearl Harbor, showed that attitudes of infantrymen were far from satisfactory. Compared with the more glamorous Air Corps, the men in training in the Infantry felt discriminated against in pay, ratings, long and gruelling training, and in obsolete training methods. They felt that the Infantry was being use as a dumping ground for healthy but stupid soldiers who could not qualify as specialists in the Air Corps or Service branches—an allegation, incidentally, which was pretty well confirmed by statistical analysis of the qualifications of soldiers in various branches. The Army Ground Forces, as far as we know, paid almost no attention to these studies, though the reports may have stimulated efforts through posters and other publicity to sell the idea that Infantry was a good branch to be in. Basic to the problem was the selection factor and the Army, neither in its publicity nor in its practice, made any pretense that an Infantryman, like a Marine, was a picked man.

Time passed. It presently became evident that the Infantry would have a much larger role to play in the War than some of the air-minded and mechanized-minded soldiers had thought. Our studies showed no improvement in attitudes of Infantrymen in training, and not much attention was paid to the findings, until the Chief of Staff

personally read one of the reports. Thereupon he ordered the Commanding General of Ground Forces to make an immediate staff study, with recommendations. Out of this staff study, in which our research unit participated, came a series of new policies, calling for increase in pay and ratings, the institution of the Expert Infantryman's badge and the Combat Infantryman's badge, and an intensive publicity program. That these policies improved attitudes slightly was later demonstrated, but by this time it was too late to attack the problem at its fundamentals—that is, to sell the idea, by showing that it was in fact true, that in some respects the Infantryman, like the Marine, was a picked man. If there had existed a close working liaison between Ground Forces and the research from the beginning, it is at least possible that the story of the Infantry could have been a different one.

Contrast this with our experience in connection with demobilization plans. Members of our unit had close working relationship with officers responsible for drawing these plans well in advance of victory. The problem which the Army expected to face was unprecedented. After the defeat of Germany, it was necessary to assume that at least another year would elapse before the defeat of Japan, and that not all the soldiers in uniform as of V-E day would be needed in the Pacific. Hence, with a war still going on, several million soldiers would be discharged. Who should these soldiers be?

One school of thought favored blanket discharges on a unit basis. The obvious advantages would be that this would keep the remaining units intact as going organizations. Another school of thought pointed out that most units overseas comprised a mixture of worn-out veterans as well as fresh replacements. To discharge men on a unit basis would involve sending home thousands of men with relatively short service. This, it was felt, would be utterly destructive to the morale of those remaining, especially if, as had been further suggested, men who had not even completed their training in the United States were to be among the first discharged in order to obviate the need for further training. If, however, the second school of thought was to prevail, how would the priority of discharge be determined? Members of the Research Branch proposed that the best results would be obtained if the priority of discharge was one which seemed most just to the majority of men, provided it did not wreck military plans. The planners bought this idea, and as early as the winter of 1943–1944, the Research Branch was polling soldiers to get their ideas about who should get out of the Army first after the defeat of Germany. This was not easy, because opinions were not sharply structured at this time. Continuing studies were made and the priorities adopted by the Army involved the four factors which studies of the men had shown to be most important in their minds—length of time in the Army, length of time overseas, extent of combat service, and number of children. The program was carefully explained to the soldiers and to the public and was accepted as just by the majority of both, as polls showed.

The plan was designed to minimize, not eliminate, complaints. And it had defects. Ideally, one should have been able to use a more accurate measure of combat experience than merely the number of campaign stars and decorations, and some of the combat infantrymen overseas were quite properly burned up about this. But, in general, all polls taken showed that the majority of soldiers at home and overseas thought the system a fair one and this was true even amid the explosive situation caused by the alleged slowness of demobilization. It will be remembered that there were some near mutinies which were reported in the press. These were concerned with the *rate* of discharge, not the *order* of discharge. If, in this highly charged atmosphere, the men had also been convinced of favoritism or injustice in the priority system, it is not pleasant to imagine the consequences, especially if relatives at home had shared the conviction.

The important thing to note for our purpose today is that this contribution of research to Army policy—in fact, to public policy in general, for the whole American people were vitally and directly concerned—would have been impossible if the research people and the policy makers had not been in intimate contact well in advance of the announcement of policy and had not kept in contact from beginning to end.

The more I have seen of the problems of putting research into action the more sympathy I have had with the policy makers who sometimes are resistant to research. The research man has a tendency to fall between two stools. Either (*a*) he will not translate his results into an idiom which the policy maker understands and state briefly and lucidly the implications for policy, or (*b*) he will recommend a policy which must be based not

alone on variable x dealt with in the research, but also on variables y and z which he does not know any more about than the policy maker. This is only in part the policy maker's fault in demanding recommendations which go far beyond the data. It also is the researcher's fault, for it is all too easy for the researcher either to retreat from the field or to become so entranced with his special data that he overlooks other factors which the policy maker must take into account.

In the War Department we committed both types of errors, but the first much more frequently than the second. Some of us were more concerned that our research be presented in full, with all qualifications in the footnotes, than we were with making the implications intelligible to the user. If there is a worse model for a report to a policy maker than a Ph.D. thesis I have not discovered it. Less often, but occasionally, some of us went too far in recommending specific courses of action when we commanded only a fraction of the necessary information.

Out of this experience, I am convinced of the need for a new kind of professional man—who might be thought of as a research broker. He needs to be skilled in research and also wise in statesmanship. He would not necessarily do research himself, but he would discover where research is needed, help the policy makers formulate those problems on which research could bear directly, and finally interpret to them the findings of research.

All signs indicate that research by social scientists is likely to be increasingly important in furthering public policy. While I believe it essential to the success of this endeavor that researchers have early and easy access to policy makers, perhaps aided by the kind of research broker I have described, I think that it is even more essential to this endeavor that researchers be able to draw explicitly on tested social science generalizations.

Practical research can help the development of the needed basic research in several ways. First and foremost, practical results, especially if derived directly from social science and not mere accumulated common sense, will stimulate financing of better basic studies and thus raise the quality of social science personnel and the quality of social science. Second, the practical uses of statistics and other techniques improves these tools for use in more basic scientific studies. Third, practical needs and the inadequacies of present concepts for dealing with them can help direct basic research into channels likely to be most fruitful ultimately.

But there is also a threat to social science in practical research. The immediate problems are urgent and the rewards are great for the use of horse sense and skillful techniques, even if not social science, in studying these problems. The danger is that these rewards will drain off too many of the energies of the relatively few social scientists who have the combination of technical training and disciplined imagination necessary to create new and solid conceptual foundations of sociology and social psychology. It is, I feel, the responsibility of those universities and those foundations which have faith in the future to help young social scientists to resist some of these siren summonses to be of immediate practical service. By stating propositions operationally, by developing measurements to test them, by discovering crucial situations for such tests, especially by controlled experiments, these men and their children and grandchildren can build a social science which will hold its head high among our more mature sister disciplines. This is not mere dream stuff. A good start has been made.

Out of the frustrations and the successes of our war experience we emerge with a renewed faith in the practical value of social science skills and in the still greater value which these skills will have when we have developed a better body of tested propositions, whose direct engineering applications can serve mankind.

OPINION STUDIES AND GOVERNMENT POLICY

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(Read February 6, 1948 in the Symposium on Research Frontiers in Human Relations)

OPINION studies is a term commonly used to refer to results obtained from sample interview surveys. Actually the sample interview survey can be used to obtain many kinds of data in addition to measuring opinions. This scientific tool has become one of the most valuable and powerful research instruments of the social sciences. It has demonstrated already that it has great potentialities for improving the effectiveness of democratic government. I believe that within the next two decades it will alter important processes of government and will profoundly affect both political science and public administration.

Broadly speaking there are two kinds of government policy: legislative and administrative. Legislative bodies, like boards of directors of corporations, have the responsibility of defining broad policy. The executive branch has the responsibility for establishing the specific policies and programs required to implement the decisions of such legislative bodies as the Congress. The sample interview survey can provide valuable data to improve both kinds of operations: legislative and executive. It can collect data on people's desires, problems and needs, on their opinions, their knowledge and misinformation, and on their behavior (15).[1] It can show how well existing legislation is functioning and indicate remedial steps that are required, both legislative and administrative. Its application to the administrative and legislative functions of government will be discussed separately.

To provide a background for this discussion, I should like to cite the results from some specific studies and indicate how they have been used to improve the quality of government. These illustrations will concern, primarily, the use of surveys to improve administration, for that is the major application that has been made to date. Moreover, while there is evidence that polling results are exercising an important influence on legislative decisions, no legislative body in the United States as yet officially makes use of survey results.

In March 1940 the people living in and near the Vernon Unit of the Kisatchie National Forest, which is located in West Central Louisiana, organized in an effort to drive out the United States Forest Service. A small group of young men set fires which burned over a large part of the area which had been planted by the Forest Service in the preceding few years. Four young men were arrested, tried, and sentenced to terms in the Federal penitentiary. As you can imagine, there was great tension between the local community and the Forest Rangers. Most of the Rangers carried side arms and all of them felt it safer not to be in the forest after dark.

In the summer of 1940, at the request of the Forest Service, the Division of Program Surveys of the Bureau of Agricultural Economics made a survey to find the answers to such questions as: why these people burned the forest so extensively in March, why they always burned small patches of the forest in the early months of each year, and what the Forest Service could do to reduce incendiary fires.

Our interviewer was understandably apprehensive when he started on this study, but soon found that he obtained sincere cooperation from the people who lived in the forest. He used a carefully designed interview which approached the people in terms of their problems as farmers. They were asked how they were getting along, how the future looked for them and their families, what was making it hard and finally, at the end of the interview, they were asked some questions about how they felt about burning the forest and related matters.

The background of the problem was as follows: The Vernon Unit of the Kisatchie National Forest comprises a substantial proportion of Vernon Parish in West Central Louisiana. It is an area that was logged off from about 1900 to about 1920. The people who live in the area are dark-skinned, with straight hair, and are a mixture of Indian, Negro, and White blood. They form something of a cultural island, as might be expected.

During the logging days these people had ample work and fared fairly well. After the timber had

[1] Numbers in parentheses indicate references at end of article.

been logged off, the land was abandoned by its owners and was treated as open range by the people who lived in the area. These people acquired small farms of from ten to forty acres where they lived and generally grew a very small cotton crop for cash. The land is so gravelly and poor that it generally took great quantities of fertilizer to produce the cotton. The main source of food and income for each family, therefore, came from the livestock it owned. Each family had a number of razor-back hogs and of tough cattle who could live off the range. This livestock was permitted to roam and forage at will over the cut-over land.

In January or February each farmer rounded up his cattle and hogs and marked them by branding the cattle and cutting identifying marks in the hogs' ears. The method used to round up the livestock was to burn a patch of several acres when the forest was not too dry near the farmer's dwelling. After the area had been burned, new green grass shot up and the half-starved livestock would flock to it. Any cattle or hogs which appeared, if not marked otherwise, were assumed to belong to that particular farmer. By systematically burning these patches, each farmer kept the underbrush from becoming thick in the region where he lived. This provided better range for his livestock. Moreover, most of the natives firmly believed that this burning kept ticks and poisonous snakes at a minimum.

Our interviews revealed that prior to the establishment of the National Forest these people were eking out an acceptable existence. When the Forest Service, however, bought the open range and began fencing it, it caused a serious dislocation in the lives of each family. None of these families owned enough land to provide range for its cattle and hogs. As the land near each family was fenced in, the livestock had to go greater and greater distances to find adequate range. These distances, coupled with restrictions imposed by the Forest Service, which prevented the burning of any land, resulted in each family losing the bulk of its livestock. Since the livestock was a major factor in the livelihood of these people, they were frightened and bitter.

The local people, generally referred to by the Forest Rangers as "natives," could not understand how the Forest Service could buy the open range and fence it. This land had always been an open range in their eyes and they had used it in this fashion for years and years. It was land that God had given to all of them to use and the Government had no right to take it from them.

After the fenced in areas had been planted to long leaf pine and the trees had obtained a small start, the Forest Service began permitting cattle but not hogs to graze in these areas. Hogs were not permitted because they uprooted the pine seedlings to eat the roots. A flat fee of a few dollars was charged for each head of livestock that grazed in the enclosed area. This fee, in addition to the grazing charges, included membership in a stock improvement association, which, under the management of the Forest Service, was to bring bulls of especially good strains into the community to improve the livestock. But this improvement program was forced on the people, not sold to them.

This entire program met with opposition by the local people. They did not believe the Forest Service had the right to fence the land or to charge them a grazing fee. They felt that they could not afford to pay this fee out of their meager cash income and they were convinced that any attempt to improve their livestock would produce less hardy animals who could not survive the rigors of heat, ticks, and open range.

When the Forest Service brought in C.C.C. boys from St. Louis and other distant points to do the work in the forest, and refused to give any of the local people jobs, these people had all the evidence they needed. They felt that an attempt was being made to destroy them and drive them from the region. In desperation they staged a "Whisky Rebellion" by burning the forest.

Our survey clearly demonstrated that the purchase of the land and the replanting of the Forest were done with little appreciation of what it would do to the people involved. Excellent planning and management were done so far as silviculture was concerned, but there was no planning for the people whose lives were so drastically affected by the program. Even the signs and instructions posted in the forest were written in a vocabulary that was not understood by the local people.

The results of this study led the Forest Service to change its policy and program in this and similar forests. The Forest Service requested that the person who conducted the study be detailed to them for a period of time to assist them in applying the results. This man spent several months working with regional officials and through a series of training sessions presented the major find-

ings of the study to every ranger in the Southern region.

The second illustration concerns one of the studies that we did for the Treasury Department (*27*) to improve the War Bond Drives. Immediately after the Second War Bond Drive we interviewed about 1,800 persons to appraise the drive and particularly to discover how to improve the Third Drive. We discovered a number of important points about who bought bonds and who did not and why. The most important finding, however, concerned the effect of personal solicitation.

In planning the study we felt that personal solicitation might be an important variable affecting bond buying. Consequently, at one point in the interview we specifically asked the respondent whether during the drive he had been asked personally to buy War Bonds. At another point in the interview we asked how many bonds had been bought during the period of the drive and how many bonds would be bought ordinarily in that period of time.

When respondents were asked why they bought War Bonds, most of them gave a patriotic reason in answer. They would say, for example, that the government needed the money to buy war equipment. Only a small proportion answered that they bought because they were asked to buy and yet when we analyzed our data we found this reason to be very important. We found that of all gainfully employed people—that is people with incomes—25 per cent reported that they had been asked personally to buy War Bonds. When we grouped these people together we found that 47 per cent of this group bought *more bonds* during the Second War Bond Drive than they had been buying. Among the three-fourths of gainfully employed persons who had not been asked to buy, however, only 12 per cent had bought more than usual.

We found that this relationship between bond buying and solicitation held for every income group, every occupational group and for every geographical region. No matter how we grouped our data, we found that among those who were asked to buy there were about thirty-five percentage points more buyers than among those who were not asked to buy.

These results were called to the attention of Mr. T. R. Gamble, Director of the War Finance Division, with the strong recommendation that a major effort be made to increase the amount of personal solicitation in the Third Bond Drive. The War Finance Division made this an important objective. They presented our results in regional meetings and published a report making these findings available to state, county, and local war bond committees.

In our survey following the Third War Bond Drive, we found that personal solicitation in the Third Drive had been doubled: 50 per cent of all gainfully employed persons were solicited. In this drive, 59 per cent of those who were personally solicited bought more bonds than usual. Among those who were not personally asked to buy, only 18 per cent bought additional bonds.

Sales of bonds to individuals were, of course, the major purpose of the War Bond Drives. This one finding on the importance of personal solicitation and its application in the Third War Bond Drive increased materially the sale of Series E Bonds and the total sale of all bonds to individuals. The doubling of personal solicitation in the Third Drive over the Second almost doubled the amount of Series E Bonds that were sold. In the Second Drive one and one-half billion dollars of Series E Bonds were sold, two and a half billion dollars were sold in the Third. Throughout all the bond drives, there was a close relationship between the amount of solicitation and the total amount of bonds sold to individuals.

Another broad program of research which we started in the Division of Program Surveys and are now continuing in the Survey Research Center consists of the Surveys of Consumer Finance for the Federal Reserve Board (1, 23). These surveys are influencing both legislative and administrative policy. Some economists, notably Rolf Nugent, who had seen our studies on the purchase and redemption of War Bonds, suggested that there would be great need for data on what people would do with their savings after the War. They pointed out that the United States would face an unprecedented situation at the end of the War. The American people would own between three and four hundred billion dollars in savings of which over one hundred billion would be in such liquid forms as currency, Series E Bonds, checking accounts, and savings accounts. If the American people suddenly decided to spend any appreciable part of these liquid assets, the effect upon our economy could be distastrous. It was exceedingly important, therefore, to obtain data that were not then available, namely data which would answer such questions as:

1. Who owns these liquid assets, i. e., how is the ownership of these assets distributed through the populations? Are they widely held or are they concentrated in the hands of a relatively small group of people?
2. If the ownership of liquid assets is concentrated to any appreciable extent, how can the groups who hold these assets best be characterized? Is it related to income, age, occupation, education, or other similar variables?
3. What plans do the owners of these assets have for the use of the assets? Do they plan to draw on them? Will they be drawn on to invest in other forms of savings, to buy consumer durable goods, or for similar purposes? If so, when, in what amounts, and for what purposes?
4. What affect will these savings have on people's plans for further savings? Will these holdings make people feel secure and cause them to spend more of their income than would be the case otherwise?

We felt reasonably confident that we could obtain answers of satisfactory accuracy to these questions with one important exception. Our experience had indicated that it might not be possible to obtain accurate data on the holdings of currency. Our confidence that most of the data could be obtained was based on the results that we had obtained in the studies for the Treasury Department. In those studies we found that we could ask respondents about family income and expand our data to national estimates that corresponded quite closely with the aggregate estimates of the Department of Commerce. Our results, based on samples of from 1,500 to 2,500 generally, were from 5 per cent to 15 per cent low. Similarly, our results on the purchase, ownership, and redemption of Series E Bonds checked closely with Treasury data. For example, during the last half of November 1945, we interviewed a national sample of 2,300 persons who received income. We asked these respondents whether they owned any Series E Bonds and, if so, how much. The total they reported owning amounted on the average to $590. At that time there were an estimated 51 million income receivers. Fifty-one million times $590 yields an estimate of $30,090,000,000 of Series E Bonds outstanding. The Treasury report on December 1 showed the actual figure to be $30,263,000,000.

After a pilot study had demonstrated that data of satisfactory accuracy could be obtained, the Federal Reserve Board authorized a study on a nation-wide sample of 3,000 interviews. This study was made early in 1946 and obtained income and savings data for the year 1945 and the plans and expectations of respondents for 1946. The results of this study were published in a series of articles in the *Federal Reserve Bulletin* in the summer of 1946.

The study proved to be so valuable that the Federal Reserve Board asked us to conduct a similar study last year and a third annual Survey of Consumer Finances is being conducted at the present time.

To illustrate how these data were used for policy decisions, I should like to quote a statement made by Dr. Ralph A. Young (28), Assistant Director of the Federal Reserve Board's Division of Research and Statistics. Speaking of the first nation-wide Survey of Consumer Finances, Dr. Young mentioned several uses of the survey data for policy purposes. Three of the uses he mentioned are:

> A final important value of consumer financial surveys is in connection with bank credit, monetary, and fiscal policy. Under present conditions, effective regulation of the money supply by the Federal Reserve is no longer possible on the basis of the flexible application of its traditional instruments. Individuals and businesses hold approximately 140 billion dollars of liquid assets that can be used at will to increase current expenditures, and conversion of bond holdings into cash can serve to increase the money supply, both directly and indirectly. . . .
>
> It was a matter of not inconsiderable importance from the standpoint of monetary and fiscal policy to know the relative concentration of liquid asset holdings by income groups at the beginning of 1946 and to find that holders for the most part were not inclined to spend them. It was also reassuring to find that the inflationary threat of this potential money supply may not have been as serious, even though serious enough, as had been thought by many.
>
> Current surveys of consumer finances also have direct usefulness for central bank policy in selective credit areas. The 1946 survey showed the possibility of a sizable conversion of liquid assets into investments, which confirmed the desirability of maintaining reasonably high margin requirements to prevent securities from becoming an inflationary menace to the whole economy.
>
> The 1946 survey was also useful in connection with consumer credit regulation. It made possible rough estimates of consumer demand during the year for durable goods and for instalment credit to finance purchases of such goods. It further showed

which consumer groups were impatient to acquire durables and to incur debt to do so. This information indicated that strong inflationary pressures would be present during the year in the durable goods markets, which were characterized by short supplies, and pointed to a continuing need for restraining "deficit spending" by consumers, i.e., consumer credit expansion, so far as possible until conditions of short supplies were remedied.

If there were time, there are many other illustrations that could be given to show the wide variety of problems where surveys have proved useful. Studies have been made, for example, which showed what needed to be done to help farmers increase the production of such seriously needed food stuffs as dairy products, vegetable oil crops, feed grains, and meat. These studies made for federal and state agencies helped them to determine policy and plan programs and often led to important changes both in plans and actual operations. Other studies were devoted to the problems of rationing and price control and preventing an excessive inflation in land prices. Still others dealt with the problem of increasing the knowledge of nutrition among the public generally and its application to improving food habits and health. Other studies were devoted to postwar problems, such as the study of the difficulties that veterans encountered in returning to civilian life. The results of this latter study were used in establishing the counselling programs that were designed to help veterans make the transition.

The wide range of studies that have been mentioned indicates the broad scope of application of sample surveys to the problems of government. While the examples have dealt with the activities of the Federal Government, the tool is equally applicable to all levels of government: local, state, national, and international.

I would like to turn now to the specific consideration of some of the functions which survey research can perform for the *executive* branch of government.

The legislative branch frequently asks the executive branch for recommendations with regard to legislation. This requires that the executive branch have accurate information on the real needs of the public. It is not sufficient to know what particular pressure groups are clamoring for. Precise knowledge of the genuine wishes and desires of the public generally is required. The only economical and accurate way for the executive branch to obtain this information is through sample surveys.

Legislation, particularly that dealing with national problems, must necessarily refrain from being too specific in order to meet the wide variety of conditions that exist in various parts of the nation. Top administrators, as a result, frequently work within very broad limits and are faced with the necessity of establishing broad operating policies and of choosing between various program alternatives. Here again sample surveys can immeasurably improve the efficiency of government and make it more responsive to the desires of the people. To make intelligent decisions on broad matters of policy and program, administrators need answers to such questions as: what understanding does the public have of the problem; how does this vary in various parts of the public; and what degree of support can the different alternative policies and programs expect to obtain?

After broad policy decisions have been made, it is necessary to decide on the specifics of a particular program. In the planning stages, sample surveys can obtain evidence on the relative acceptability of different alternatives and can indicate the degree of support or resistance that each alternative will receive and from what segment of the public. Surveys can indicate also how the program should be presented and how it should be administered to obtain maximum cooperation and support.

The acceptance and support that a program receives depend not only upon its soundness and how well it meets the needs of the public, but also on the understanding that the public has of the problem and the information given them about the program. Fundamentally sound programs have failed because of misinformation and ignorance on the part of the public. Survey research can provide administrators with data on the amount and kind of misinformation or ignorance that exists among the different segments in the population with regard to any program or problem. But even more important, they can point to efficient ways for remedying existing ignorance.

The timing of a program and of its parts often involves difficult administrative decisions. Survey data can be useful in these decisions.

Useful as surveys can be in deciding on new policies and planning new programs, they are even more valuable to administrators in appraising and improving existing programs. Serious administrative errors are often made by incorrect informa-

tion on how a particular program is actually functioning. Survey research can help to minimize these errors by periodically securing accurate answers to such questions as:

How effectively is the program working in the judgment of the public generally? What support does it have?

What segments of the population feel that the program is working well? What segments feel that it is working poorly? Why?

What suggestions or criticisms do people have? What improvements do they seek? Why?

How well informed are people about the program and its objectives? What groups are uninformed or misinformed? How can they most readily be informed?

If the program involves participation on the part of people generally, what kinds of people are participating? What segments of the public are refusing to participate? Why?

At times various governmental agencies have the task of conducting a program to educate or inform the public, or large segments of it, on some matter of importance. The Nutrition Education program of the Federal Government is an illustration as are the widespread educational programs of the Federal and State Agricultural Extension Services. In this field of governmental activity, survey research has demonstrated that it can help administrators to improve substantially the results obtained per dollar expended. Studies have shown, for example, which approaches and appeals are most effective in interesting people in better nutrition and health and how best to change food habits (25). Director M. L. Wilson of the United States Extension Service is now urging the State Extension Services to put as much as 5 per cent of their budget into studies to evaluate and guide their educational efforts.

This tool can be used also to measure the morale of any agency, and to determine what influences are producing high or low morale (12, 18). The morale of individual units or sections can be measured as well as that of the entire agency. By providing an administrator with data on where morale is poor and why, and what to do to remedy it, sample surveys can help to improve the efficiency of government employees.

As I have endeavored to indicate, the sample interview survey has wide applicability to the problems of the executive branch of government. It can provide administrators with facts based upon measurements to help guide them in their decisions. Fortunately for good government the use of the sample survey is increasing. There certainly is no longer any need for administrators to rely on the impressions and spotty knowledge of a few friends, the reports of pressure groups, or other fragmentary sources of information to guide policy and operating decisions. To be guided by such incomplete and unreliable information, when much more accurate data can be obtained, is inexcusably wasteful.

I have been discussing how survey research can be used by the executive branch of the government. I should like to turn now to a consideration of how survey results can be valuable to the legislative branch.[2]

Election results are by no means a clear indication of what the people want so far as the treatment of specific issues are concerned. The results of elections are the composite outcome of many influences, including many different issues and the personal qualifications of the candidates (8). Consequently, in any democratic society there is great need to know how the public feels on major issues.

Congressmen and legislators generally agree that it is necessary to know the opinion of the public on issues under consideration, but until recently, they have had no alternative but to accept grossly inaccurate and crude measures. Even though more accurate measures can now be obtained, habitual reliance on the old methods continues. Many Congressmen seek information on public opinion by systematically following newspaper editorials, assuming that these editorials present with reasonable accuracy the opinion of the public. Another method used by many Congressmen is to visit with "representative" leaders in their district. A substantial number of Congressmen state that they rely heavily on the mail they receive as a guide to public opinion (14).

[2] Judge Herbert F. Goodrich of the United States Court of Appeals, Third Circuit, suggested, during the discussion period which followed the presentation of this paper, that the results of sample surveys could be of value also to the judicial branch of government. He pointed out that the court is often called upon to interpret legislation. In this process the court seeks to discover the intent of the Congress when it passed the legislation in question and to interpret the law accordingly. Judge Goodrich stated that it would help the court greatly if the Congress were to use survey data in deciding upon legislation and would place these data in the record. The court would then have available a much more complete body of information on the intent of the Congress to aid it in its interpretation of the particular legislation under consideration.

This reliance on newspaper editorials, discussions with "representative people" and personal mail continues in spite of research showing that these methods are extremely inaccurate measures of public opinion.

Perhaps the most surprising method used by some Congressmen for keeping abreast of public opinion is the mail questionnaire. Careful methodological research has shown that mail questionnaires can be seriously inaccurate unless used with very careful controls such as a well designed sample and interviewing a sub-sample of those who do not answer the mail questionnaire. Unfortunately these controls are not used by the members of Congress who now employ mail questionnaires.

One reason why legislators fail to use survey research is their lack of knowledge of what this new scientific tool is and what it can do. Another may be their fear of technological unemployment. They do not want the ancient art of politics altered by science. There are many other reasons also that could be mentioned, but I should particularly like to discuss two important reasons. The first of these is that many Congressmen and other legislators at times distrust the results of published polls. The second is that Congressmen generally resent the role of being merely a rubber stamp that endorses and acts upon poll results. I shall discuss each of these points separately.

No doubt, Congressmen have been influenced in their attitudes toward the published polls by criticisms that have been made of these polls (13). There is evidence that suggests that some of the recognized polls occasionally have not been as objective as the public can properly expect. Nevertheless, it is important not to discount the large amount of important and accurate results that these polls have published and the value of these results for governmental policy (7, 16).

Underlying the uneasy feeling some Congressmen have about the accuracy of polls is a serious and important criticism. They rightly reason that the wording of a question can exercise a major influence on the results obtained. And they suspect that pollsters can obtain any answer that they wish by the way they word the question. This is a bit extreme, but there is ample evidence that the wording of the question and the sample that is used profoundly influence the results that are obtained (3, 5).

Congressional fears, as well as scientific evidence, clearly point to the need for a change in practice by many polls. To obtain reactions to a single question and to say that these results adequately represent how the public feels about an issue is unwarranted from a scientific standpoint. The replies to a single question cannot give an adequate measure of public thinking on any important issue (6, 21). Any problem generally has several major dimensions and each of these needs to be measured accurately. Moreover, sound research demands that opinion studies be designed so that more than one measurement is made of each important variable. Independent checks within a survey are essential. All this can be done, and when it is done, Congressmen and the public generally will have more confidence in poll results.

Another step that would help to establish confidence in each poll would be the publication of a clear, full statement of the sample that has been used. The results of any poll is influenced materially by the sample employed and a description of the sample should be available for all who may be interested.

It is understandable that many Congressmen resent the suggestion that they should behave like automatons in following and acting upon polling results. There is great danger to democratic government in this concept. It is unfortunate that polls have been used in this manner by a few legislators.

This point of view was well expressed in a recent issue of the *New Yorker* (19):

> We have often wondered how journalism schools go about preparing young men and women for newspaperdom and magazineland. An answer just came the other day, in a surprising form. It came from California via *Editor & Publisher*. We quote:
>
> > "San Francisco—Public opinion polls are scientific tools which should be used by newspapers to prevent editorial errors of judgment, Dr. Chilton Bush, head of the Division of Journalism at Stanford University, believes.
> >
> > "'A publisher is smart to take a poll before he gets his neck out too far,' he said. 'Polls provide a better idea of acceptance of newspaper policies.'"
>
> We have read this statement half a dozen times, probably in the faint hope that *Editor & Publisher* might be misquoting Dr. Bush or that we had failed to understand him. But there it stands—a clear guide to the life of expediency, a simple formula for journalism by acceptance, a short essay on how to run a newspaper by saying only the words the public wants to hear said. It seems to us that Dr. Bush hands his students not a sword but a weather

vane. Under such conditions the fourth estate becomes a mere parody of the human intelligence, and had best be turned over to bright birds with split tongues or to monkeys who can make change.

Our democracy—any democracy—is doomed if the leaders become mere followers of public opinion. There is need for genuine leaders in our political life who will discuss major issues, present them to the public with a clear statement of what each problem involves, the possible alternatives and the relative advantages and disadvantages of each. Sample surveys can perform a very valuable service to the legislator who is a *real* political leader. Instead of merely obtaining data showing whether a majority favor or oppose a particular policy, survey research can perform such functions as the following for each issue or problem. It can:

1. Provide evidence showing what policy the public actually wants pursued on the given matter.
2. Show what portions of the public wish to have a given policy followed and what portions prefer other policies and why.
3. Obtain data on the level of information that different parts of the public have on the problem, the sources of this information and the best methods for increasing the level of knowledge and understanding among each part of the public.
4. Secure results to show the relation between the knowledge and understanding of the problem by different persons and the opinion that is held.
5. Obtain evidence on the underlying motives that are influencing the opinions held.

Our democracy will take a long stride forward in its effectiveness when our legislators and political leaders use survey research to obtain such material and then use this material honestly to foster understanding among our citizens of the basic issues of the day. Moreover, this same material will enable legislators to perform their functions far more effectively. Let me illustrate by discussing the British Loan.

From December 1945 until after the Congress passed the British Loan in the summer of 1946 surveys showed that more people opposed the loan than favored it. Most of the polls published data only on the question of whether people were in favor or opposed to the loan. Since a plurality to a majority were opposed, then the Congress, if it were strictly to follow public opinion, should not have passed the loan. Fortunately the Congress used better judgment.

There is convincing evidence that the answers to the direct question of attitude toward the British Loan was not an adequate measure of the policy that the public wished the Congress to follow. Farmers feared a collapse of prices for agricultural products and were very eager to have the government take every step possible to maintain agricultural markets and prices (*25c*). Industrial workers similarly feared a drop in business with resulting loss in income. They, too, wished the government to take steps to help maintain business activity and purchasing power. Both farmers and workers were far more concerned about their income and economic welfare than they were about the British Loan. The loan, if it interested them at all, was generally a peripheral matter.

It is not surprising, therefore, that there was a significant relation between the understanding that people had of the loan and their attitude toward it (*20*). Those persons who had even the vaguest understanding that the loan was to help stabilize the economy of the British Empire were overwhelmingly in favor. On the other hand, those who knew little about the loan or the conditions requiring it were opposed to it because "the British owe us too much" and "the loan won't be repaid."

It is clear that measurements of all the relevant opinions and desires with regard to the British Loan indicated that the public perferred the loan to be made. It is also clear that the data from a single question can be misleading. Moreover, the British Loan illustrates an important differentiation that must be made by legislators in the use of survey results. It is essential to differentiate between policy on the one hand and the specific ways of implementing policy on the other.

At times polls have asked specific questions on alternative ways of implementing policy. To ask these questions and to publish them with the inference that this is the program that the Congress and the administration should follow is open to serious question. Frequently, the public generally lacks the technical information required to choose the best alternative. The wisdom of public experience and public thinking can indicate the general policy to be pursued but the help of technical experts is often required to devise the best method of implementing the will of the public.

The sample interview survey either in the form of published polls or in the form of special studies

for legislative bodies can be a valuable tool to help legislators. It can show them what the real desires and wishes of the public are; it can help to disarm special pressure groups; it can show where there is need for greater public information and assist legislators in many similar ways. In serving all these functions, survey results should be used as a guide to the deliberations and judgments of the legislators in much the same way as other factual data are used.

There are several ways in which a sample survey research operation could be organized to serve the Congress. The form of organization necessarily would need to assure complete objectivity and scientific accuracy regardless of which party is in power. It could be established under the supervision of a committee appointed jointly by the National Research Council and the Social Science Research Council. Or it could be organized under the direction of a group of cooperating universities which have particularly strong social science departments.

There are other alternatives but whatever way is used, it is highly desirable that an advisory committee of outstanding experts be established. This committee should consist of persons who are recognized as technically competent by the professional and scientific societies that are actively concerned with the methodology of survey research.

There is not time to discuss the methodology involved in survey research. Virtually all of it has been developed in the past ten years, much of it in the last five. Some of the recent developments in methodology can significantly improve the accuracy and value of the widely known polls. Improvements are possible in sampling. Area and other random methods are appreciably more accurate than the quota method commonly used by polling organizations. Improved procedures of survey design, interviewing, and analysis are also available. Although there has been a steady shift in the direction of using better methods by all polling organizations, this shift at times has not been as rapid as warranted.

Until there is more widespread use of the best available methodology, however, polling organizations will draw erroneous conclusions and not be as useful to legislators and administrators as possible. There is real need, therefore, to encourage our major polling organizations to improve their methodology. This means that the profession itself must maintain a high level of alert self-criticism and a substantial amount of methodological research.

There is need also to do more than make legislators and administrators aware of the many valuable functions that the sample-interview survey can perform for them. They must be made to realize that this is a scientific tool and like all scientific instruments it must be used by personnel specifically trained in its use. Journalists and lawyers, for example, are well-trained for certain tasks but not for survey research. In the hands of inexperienced and incompetent persons, serious errors in results are obtained. Corresponding errors in policy and operating decisions will be made by those who rely on inaccurate data.

The sample interview survey, as a powerful research tool, can help make government both more responsive to the needs of those who are governed and appreciably more efficient. It can help our democracy to utilize more fully one of its greatest assets: self-criticism. The analysis of the war effort of Germany disclosed a serious weakness. Incompetent people were permitted to stay in important places of leadership because criticism from below was not permitted. Similarly unsound policies were pursued long after their weaknesses were evident because criticism was not permitted. The sample survey, by providing a means for tapping in an accurate and unbiased manner the experience, criticisms, suggestions, and thinking of the American people, can contribute materially to making our form of government the most powerful in the world. Its power will stem from its capacity to utilize fully the experiences and thinking of all and thereby to serve better than any other form of government the fundamental desires and needs of the people it governs.

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THE NATURAL SCIENCES AND HUMAN RELATIONS

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(*Read February 6, 1948 in the Symposium on Research Frontiers in Human Relations*)

THERE are two ways of interpreting the somewhat ambiguous title of my address. I may either call your attention to the effects of progress in the natural sciences on the phenomena of human relations, or else discuss the ways in which the study of the natural sciences interacts with the study of human relations. On the ground that it is slightly less trite, I shall deal largely with the latter aspect of my subject.

The first and most obvious way in which the development of natural science has influenced the study of human relations relates to methodology. The experimental method developed first in respect of the natural sciences has been more or less taken over into the social disciplines. In the field of social studies, the most important aspect of the so-called scientific method is its objectivity. It is desirable, but not essential, that the scientific method include resort to controlled experimentation. Such controlled experimentation is not always possible even in the natural sciences; astronomy and geology, to name two examples, are primarily observational sciences. Sociology and cultural anthropology are similarly confined to be observational sciences, yet there has so far been far less carrying over into these fields of results and methods originating in the laboratory.

It is often asserted that laboratory investigations on societies, under controlled conditions, are simply not practicable. This is probably an overstatement. Beginnings have been made in studying the behavior of human beings in groups under the influence of more or less controlled conditions. We can expect these techniques to become highly fruitful as experience in their use is gained, and, with sufficiently careful design of experiments and interpretation of results, we can even anticipate that the findings made with respect to such miniature artificial societies will have some verifiable bearing on the nature of human behavior under the circumstances of real life. However, it is undeniable that the genuinely scientific examination of the problems of human relations has scarcely begun, and that much of the work done in the past is crude by scientific standards, and sometimes so lacking in objectivity as to be misleading.

True objectivity is hard to maintain in the social sciences, not only because of the great difficulty of meaningful controlled experimentation, but also because of the familiarity and obvious personal significance of the subject matter and of the problems. The problems of social science are not only important to the social scientist (as physics is to the physicist); it is clear to every thinking man that they are of immediate importance also to him. (This is true also of physics, but that fact was not widely noticed until August 1945, and is still sometimes overlooked.) In most cases, several varieties of prejudice and dogma offer ready answers to the question which the honest social scientist is trying to investigate with an open mind. This circumstance provides a measure of the degree to which the social sciences should be given support, but it also has, in the past, occasionally made it difficult to maintain financial support for the objective investigator who has trod on a tender dogma. Of course, natural science has had similar difficulties with dogma. We remember the imprisonment of Galileo. The social sciences may overcome their troubles of this sort as did the natural sciences—by building up such a useful structure of verifiable knowledge that even the prejudiced amateur will admit the superiority of scientific principle to his dogma.

James T. Farrell is said to have defined a sociologist—in a more direct idiom than the one I use—as a man who can spend fifty thousand dollars to learn the address of a house of ill fame. This summarizes a type of social research that has sometimes been met in the past: a stupid multiplication of formless data, in the notion that such activity is "scientific," without the leaven of careful hypothesis, objective test, and wise interpretation. I have the impression that the fraction of effort devoted to investigating human behavior on this fruitless and sterile basis is declining steadily, as the experience and standards of the social disciplines increase.

My knowledge of the detailed character of current research on problems of human relations is so meager that, particularly in this company and on this program, I must speak no further on this first point at which natural science impinges on human relations. My thesis is simply that research in human relations, like all today's research, owes a debt to Galileo. When the social sciences are properly pursued, they are genuinely sciences: empirical at present, to be sure; observational, for the most part; experimental, when they can be—but always objective and never dogmatic.

Let us turn now to the second plane on which we can examine the influence of the natural sciences on human relations. On this plane we contemplate with awe and disquiet the fashion in which modern technology, observably the daughter of natural science and the scientific method, has altered the nature and scope of the relations between men in the world as it is. The astounding growth of technical achievement in our time is no less frightening because it is thoroughly familiar. Many of you here tonight will remember a time at which there were no aircraft except hot-air balloons; some of you will remember when there were no automobiles. I can myself remember when radio telephony was a laboratory curiosity, accomplished by putting a carbon microphone in series with a Poulsen arc. The gap between scientific discovery and engineering exploitation is very narrow now. If you are not impressed by the brevity of the six and a fraction years elapsing between the discovery of nuclear fission and the incident at Hiroshima, consider this: I am of precisely the same age as is the idea that an atom has a nucleus at all.

Communications and transportation have advanced to the point at which the world is genuinely one community from the standpoint of principle, and nothing except artificial barriers erected by men can prevent direct and ready intercourse between individuals, no matter where they happen to be or who they are. John Donne's emotional "I am involved in mankind" is today nothing but a sober statement of physical fact. Jules Verne's *Around the World in Eighty Days* was published in 1872, and Verne himself lived until the year 1905. In 1947 the world was circumnavigated in something less than eighty hours, to advertise ballpoint pens. I confidently expect to live to see the the first voyage to the moon, if not beyond.

What all this means for our present topic is that the apparatus of human relations—the framework within which men have relations with one another—is constantly altering. The speed with which it changes is measured by the speed of growth of science and technology, in the absence of repressive legislation, which has often been suggested, but which we have not yet got, and about whose practical wisdom I have grave doubt. The rapidity of growth of science and technology seems to be increasing at an exponential rate, as a more or less expectable result of the fact that the knowledge already won increases our awareness of new problems, and increases also the power of the methods that we have at hand to attack them.

Thus the basic emotions governing human intercourse—greed, lust, love, hate, idealism, and the rest—play their roles against a constantly shifting background and must use different physical tools for the achieving of their ends. The basic emotions themselves have hardly changed at all throughout recorded history. There is excellent evidence that they have not changed in the last two thousand years. In *The Nation* for October 8, 1938, Archibald MacLeish altered a long quotation from Thucydides to make it apt for 1938, simply by moving the locale to an office building in Prague, and substituting "Nazis" for "Athenians," "Czechs" for "Melians," and "British" for "Lacedaemonians." The emotions underlying government—and, one is tempted to conclude, the practices of government themselves—have not altered significantly in two millennia. But the Athenians' wars were decided by brave men approaching within arm's length of one another to thrust sharp bits of metal into one another's vitals, while the Nazis' wars were decided partly by brave men who killed one another in complicated ways at distances up to twenty miles apart, and partly by highly coordinated going-on in mines and factories and laboratories. The next war, we are told by people who ought to know, will be decided by men (who no longer need be brave) pressing buttons some thousands of miles away from the men they wish to kill.

I wish to suggest that this constantly changing background against which human relations are carried on implies that we must diligently attempt to make fundamental social science research deal with the relatively durable human properties, not with the detailed expression of such properties in a given fleeting framework. It may even be that we have here a criterion which can distinguish between fundamental social research and social engineering. In so far as a given research project is

divorced from the fickle technical apparatus of current civilization, to deal with the basic behavior of human beings, it is a fundamental project; in so far as it deals with a current problem importantly involving the current technical apparatus of human relations, it is social engineering. The distinction between fundamental science and engineering in the field of interest to the natural sciences has been a useful one, not for the drawing of conclusions as to which is the "better" or more elegant, but for the clarification of goals and for the improvement in the directness with which curricula of study can be laid down in the two fields.

My earlier remark that we must strive to make social science research deal with durable human properties was not meant as derogation of social engineering, but as a recognition of the fact that it is always easier to gain lay support—which, for all practical purposes, means any support—for a specifiable program of engineering studies than it is to gain such support for fundamental research. This is true in physical science as opposed to its engineering applications, and I have no doubt that it is true in social studies as well. Both types of work must be prosecuted, fundamental studies as well as engineering; and, to be sure that both are prosecuted, we must press harder on the fundamental studies than we need to do on the engineering.

There is plenty of evidence that large-scale social engineering will be easy to support; in fact, it is currently being carried on, on an entirely empirical and frankly biased basis, by such agencies as labor unions (witness Petrillo's battle with the technology of communications) and radio broadcasters (witness the dependence of the character of radio entertainment offered on the results of polls intended to determine whether such entertainment is wanted). The two specific examples I have mentioned emphasize how important it is to have social engineering put on an objective and disinterested basis as soon as possible; for the results of bias in this important area of ideas can be completely destructive of the truth.

It is fully understandable that biased attempts at social engineering are made, for the threat of technological unemployment, to mention only one unpleasant issue of the rapidly changing technical scene, is bound to generate sentiments of considerable warmth among the human beings who contemplate it. It is equally understandable that the first reaction of an interested group to the rapidity of present technological change is the feeling that such change ought not to be permitted, ought to be legislated against. One often hears the assertion: "We ought to have a moratorium on physical science, so that the social sciences can catch up."

Now, I shall try to show later that this difficulty experienced by the engineering arising from the natural sciences will soon be experienced also by the social engineering arising from the social sciences. Leaving that point for a moment, let us agree that no scientific moratorium is a permanent, a stable, or even a possible solution for the current difficulties of human relations. As the world is presently organized, agreement between nations on such a step would be immeasurably more difficult to achieve than is such international agreement concerning a single small branch of technology—the control of atomic energy. Agreement on atomic energy control appearing impossible, we must admit that a general technical moratorium cannot be achieved. That being so, we need not explore the arguments, which are convincing, that a technical moratorium would be unwise.

Thus, it is the task of the social sciences to deal with the problem of human relations not against a stable background of technical means of human interaction, but in the most general way that can be imagined; for it must be accepted that the external forms of our civilization are forever undergoing the most profound and violent change. This presents a challenge even greater than the tremendous challenge of reducing social behavior to order against a single technological background, but the larger challenge must be faced, and recognized as the fundamental problem of the social disciplines.

So far we have remarked that the natural sciences have lent some experimental methods to the study of human relations, and that they have immensely complicated, through technology, the problem of studying human relations in a changing world. I wish now to draw your attention to a fact that is doubtless familiar to you all, but one that has, in my opinion, great future significance for the social disciplines. I refer to the fact that success in the social studies, and particularly in social engineering, will raise problems that the social disciplines cannot solve.

Everyone knows that the winning of atomic energy was a triumph of the natural sciences, but one that raised social and ethical problems quite outside the field of competence of natural science.

Consider, however, the frightening circumstance that the pen is, in fact, mightier than the sword. Consider that the increasing technical competence with which media of mass communication can be managed makes it possible to sway the opinions of almost all the people by a properly conducted campaign of information—or, if you will, propaganda. Now it is one of the businesses of social engineering to learn how properly to conduct such a campaign. We know a good deal about it already, and we are learning more every day.

Perhaps we should ask ourselves: for what purpose are we erecting the new techniques of public influence? To what end are we striving to learn more about the fundamental mechanism of human relations? It seems perfectly conceivable that, with the expectable advance in our knowledge of the character of human relations, the *wish* for redress against tyranny, for example, may not exist. There is some suggestion that such a thing may have begun to happen in Russia. On good evidence, we view the internal conditions within Russia as being those of the blackest tyranny, yet the disaffection of the Russian people seems small out of all proportion to our opinion of the conditions under which they live. Is this because of the complete mastery of the channels of communication by the ruling clique? And will a similar thing happen to us? Has it already happened? How does one tell the difference between an enlightened democracy and a tyranny when the reins of newspapers, radio, and television are in the hands of the rulers? Is it bad not to be able to tell the difference? If everybody is satisfied with a given social order, this satisfaction having been produced by the manipulation of opinion by the ruling group, is tyranny going on or is it not?

These are very difficult questions, and I have no pat answers to propose for them tonight. I say only that they must be given the most profound and prayerful consideration. For, the tools that the social engineer can put into the hands of individual men—men dancing to the tune of greed, lust, and the usual human motives—are of far more profound significance than the tools put into the hands of men by the engineering that has grown out of the natural sciences. Who shall use these tools? And for what purpose? And how shall it be guaranteed that the desired issue ensues? I simply do not know.

Possibly the most alarming aspect of this whole problem is the changing way in which education has been regarded as affecting the goals of men's relations with one another. In his *Notes on Virginia,* Thomas Jefferson wrote:

> In every government on earth is some trace of human weakness, some germ of corruption and degeneracy, which cunningness will discover, and wickedness insensibly open, cultivate, and improve. Every government degenerates when trusted to the rulers of the people alone. The people themselves therefore are its only safe depositories. And to render even them safe, their minds must be improved to a certain degree.

To what degree? And who does the improving? With what ends in view? A century and a half later Aldous Huxley said:

> By the education they have received in schools and, later, at the hands of the writers of advertising copy and political propaganda, the great majority of men and women have been conditioned to believe that progressive institutionalization, controlled by private capitalists, or the state, or both together, is an intrinsically beneficent thing and at the same time an inevitable and quasi-natural development.

Is this bad? And who decides? On what bases? It is clear that our faith in the fundamental proposition that the truth will make men free has been badly shaken.

I am not wise enough, or presumptuous enough, to answer such questions. I point out only that they exist, and that they are vital. For, while the mastery of physical environment that has been put into man's hands by the growth of the natural sciences is significant, still its significance in comparison with the tools that an ordered study of human relations may create is very small. I am a friend and patron of the social sciences, and I wish them well. But, like the natural sciences, the social scences will definitely, by their very success, raise questions that they cannot solve.

We shall then hear a great popular clamor for a moratorium on the social sciences, so that ethics can catch up. I exclude ethics, since it does not deal with measurable and determinable properties, from the content of the social sciences. The proposed moratorium on social science will be as impossible to apply and as mistaken anyhow as the proposed legislation against natural science, in my opinion, but the clamor for it will be raised nevertheless. Or will it? Possibly the powerful techniques of manipulating opinion will be able to prevent anyone from puzzling over such difficult questions as the ones I have brought to your attention.

At any rate, it appears from the standpoint of 1948 that, though knowledge is undeniably power, the moral ends for which that power is used cannot be determined by the science of human relations any more than they can be by natural science. These ends and goals are in the charge of man's ethics and morality. Social science can make it clear that given behavior brings a given result, not whether that result is good or bad. We can only pray that man's morality is competent to the handling of the tools with which the social sciences will soon equip him.

THE SOCIAL SCIENCES AND THE HUMANITIES

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(Read February 6, 1948 in the Symposium on Research Frontiers in Human Relations)

THE vastness of the subject assigned me is so redoubtable that I must begin the discussion within what I feel to be relatively familiar territory, within my own field, history. If there is any frontier line between the social sciences and the humanities, then history lies across the frontier as a sort of buffer state, and some of the intellectual experiences of the historical profession may therefore be of interest. Facing an audience with members knowing more than I do about nearly every corner of this broad topic, I hope only to point out some similiarities and contrasts in the methods by which human affairs are studied.

Briefly put, my theme is that the movement to ape the natural sciences which began about a hundred or seventy-five years ago has caused much misunderstanding and sneering or coldness along the frontier between the social sciences and the humanities, partly because too much emphasis has been placed on organizing knowledge in propositions of correlation or into propositions which in a sweeping way state the whole sequence of historical development. Recently, since the First World War particularly, that ideal of being scientific—which involved I believe inadequate understanding of what was basic in the natural sciences—has been on the decline, at least in historical study. There is some danger that this shift in the climate of opinion will lead to the building of a higher barrier between humanities and social sciences. Accepting with enthusiasm the slur that history is only literature, some historians are turning their backs on social science. But there is also hope that the present questioning of the "scientific" ideal will break down the barrier and create understanding cooperation. The breaking of the barrier seems to me to depend on reaching a common understanding concerning the necessity of describing human relations in terms of motives and concerning the way to frame and test such propositions about motives. When we study past human action as a guide to future action, the most fruitful way to organize our knowledge into a general system is not through coefficients of correlation nor yet through sweeping developmental theories, but through what I will call structural propositions—models or types—which are constructed with close attention to their relation to historical realities. This method requires the resources of both the social sciences and the humanities.

Being a historian, I will use a historical approach to my theme, and take as my starting point the attitude towards science expressed by the presidential address delivered at the American Historical Association almost exactly forty years ago by George Burton Adams. The distinguished medievalist noted the rising clamor that history must be a science, or as he summarized it, the assertion that history is "the orderly progression of mankind to a definite end, and that we may know and state the laws which control the actions of men in organized society."[1] Regarding such a science Professor Adams was skeptical, but he made without reservations the following declaration of faith "All science which is true science must rest upon the proved and correlated fact." Facts are the foundations, and he exhorted his hearers to collect them and pass them on to later builders, saying "the man who devotes himself to such labors, who is content with the preliminary work, will make a more useful and more permanent contribution to the final science and philosophy of history than will he who yields to the allurement of speculation and endeavors to discover in the present stage of our knowledge the forces that control society, or to formulate the laws of their actions."[2]

For about a generation historians were comforted by this faith that facts were the foundation and that if enough were collected someone someday might be able to build with them a science of history. Then arose haunting echoes of Emerson's dictum: "Time dissipates to shining ether the solid angularity of facts. No anchor, no cable, no fences avail to keep a fact a fact."[3] In language

[1] Adams, George Burton, History and the philosophy of history, *Amer. Hist. Rev.* **14**: 229–230, 1909.

[2] *Ibid.*, 235–236.

[3] Emerson, Ralph Waldo, *Essays*, 9, Boston and New York, Houghton Mifflin, 1903 [Vol. II of the Centenary Edition of the Complete Works].

more precise, modern, and epistemological, the historian was told that not only his selection of what to study or narrate, but even his establishment of the fact itself depended on his preconceptions. The presidential address of Carl Becker in 1931, "Everyman his own Historian" breathed an atmosphere utterly different from that expressed by G. B. Adams in 1908. In the new climate of opinion, "the proved and correlated fact" was discovered to have been a theory in disguise all the time, or at least a compound partly of evidence and partly of the historian's emotions and preconceived ideas.[4] Consequently in our day historians are showing an interest in theories of history which even Arnold Toynbee has failed to satiate. The report of the Social Science Research Council's Committee on the theory and practice of historical study has also proved a best seller. It gives a broader view of developments which I am looking at only from a special angle.[5] As long as a historian could feel respectably although humbly scientific in his untheoretical collecting of facts, he was fortified to resist temptations to be literary, or philosophical, or scientific in ways requiring abstract thinking. Now that he has discovered that he is a theorist in spite of himself, he is more inclined to inspect what those who are theorists by deliberation have to offer.

To be sure, there were always some historians who deliberately theorized about history. They were seeking mainly laws of development. Although there are aspects of human affairs that seem recurrent—as was emphasized this morning by Dr. Marquis—the aspect of change which is the historian's main problem constitutes a development through successive stages or states each of which was made possible by that preceding and was prerequisite to that which followed. Accordingly, the expression "historical law" most frequently means a proposition which characterizes the direction and sequence of development. The nebular hypothesis is a historical law in this sense. Edward P. Cheyney used it thus when in his presidential address to the American Historical Society in 1923 he proposed six laws in history. His fourth law was a law of democracy, a tendency for all government to come under the control of all the people. His sixth was a law of moral progress.[6] A developmental proposition more complicated than any of Cheyney's, and one much more influential, was Karl Marx's sequence of class struggles. These broad theories describe change which is all in one direction. There is no assumption that we will go back from solid planet to gaseous nebula, from man to ape, from democracy to absolute monarchy, or from one form of class struggle to an earlier form, and start over. These historical laws or developmental propositions are not descriptive of sequences which are expected to repeat themselves.

Although the men who framed the best known developmental propositions, notably Karl Marx, claimed to be making history scientific, their "laws" were really too sweeping and did not in fact serve that end. If we inquire what is the purpose of making these developmental propositions, we find that some modest developmental theories are useful to be sure in making predictions, as is illustrated by the studies of population, but that the broader they are, the less is their predictive value. Even if the general direction of development in the past is well established empirically, there is little basis for telling what will occur in the immediate future. Economic history can say that the general trend has been towards more and more efficient production, but there have also been ups and downs. Knowledge of the general trend is not an adequate basis for short range predictions. On the other hand, if we seek to use developmental propositions to predict far ahead, we are on weak ground logically. How sound is a long extrapolation forward of a curve of development when the curve is merely a chart of observed occurrences in the past and we do not know the factors which determined the occurrences? In the long run an entirely new factor may set matters going in an entirely new direction.

If developmental propositions based on the very slight knowledge which has as yet been gained of human affairs are of such little use in making practical prediction, why have they been formulated with so much pains? Examination of the propositions themselves indicates the answer. The main value of organizing on a large scale what was known about the direction and sequence of development was, and is, to formulate a conception of where we should go, and why. Every important general theory of historical development has been shot through with assertions as to what is worth

[4] *Amer. Hist. Rev.* **37**: 221–236, 1932.

[5] *Theory and practice in historical study: a report of the Committee on Historiography,* Soc. Sci. Res. Coun. [Bull. 54], 1946.

[6] Cheyney, Edward P., Law in history, *Amer. Hist. Rev.* **29**: 231–248, 1924.

while in the world, and it has also contained statements, capable of being proved empirically true or false, concerning the conditions under which good and evil have existed in the past and could or will exist in the future. That of Arnold Toynbee, which is most popular at present, illustrates the ethical and in a measure empirical character of these broad theories of development. I do not mean to disparage them on that account, for we need systematic thinking which harmonizes and strengthens our ethical beliefs. But that is not science, or at least not what was admired as science by social scientists who were following in the trial of the natural scientists and thereby becoming less and less concerned with ethics and more and more concerned with correlations which could be used to make hypothetical predictions. Social scientists were not attracted by "laws of history" which proved to be at best ethical systems and at worst mere wishful thinking.

Meanwhile some of the young historians who in 1908 dreamed of building a science of history were treading paths more nearly parallel to those of natural scientists. They turned away from developmental propositions, focused on recurrent aspects of historical events, and sought to find in these recurrences the basis for another kind of historical law more modestly called correlations. In the very next volume after that printing G. B. Adams' presidential address, the *American Historical Review* carried an article by Frederick J. Teggart. In this article of 1910, Teggart outlined an hypothesis for explaining historical changes in ways "verifiable by the processes of science."[7] In 1939 Professor Teggart published the results of his thirty years of effort to apply strictly "the methods of science to the facts of history," in a book entitled *Rome and China.* It bore the subtitle, *A Study in Correlation in Historical Events.*[8]

By 1939 much progress had indeed been made in finding and using correlations in historical research very much as they were found and used in natural science—observing variable phenomena and noting correspondences between a change in one factor and a change in other factors. To a limited extent the humanistic historical disciplines use correlations to make predictions much as they are used in experimental science. Archeologists note the correlations of certain types of pottery with the sequence of strata in buried cities and associate particular types of potsherds with the first use of metals, other types of pottery with the various evidences of trade, and so on. With the observed correlations as his basis, the archeologist will predict what types of potsherds will be found in later excavations and in association with what other artifacts. By digging he can subject his predictions to verifications.[9] Of course predictions made by these methods are not correct 100 per cent of the time, and actually the archeologist, like any scientist, will work out predictions concerning various sites and select for excavation a mound concerning which he is in doubt, in order by his excavation to resolve the doubt.

The archeologist's use of this kind of historical prediction is more easily dubbed scientific because he deals with material remains, but the same methods are used by those whose sources are in written words. The historian who has studied guild statutes can make some pretty good predictions about what will be found in other guild statutes. Moreover, both a student of guild statutes and a student of Greek statues are in effect making statements, not about material things only, but about the people to whom their evidence is relevant. They are making predictive statements about human affairs in the past, and on the basis of a few cases forecasting what will be found in other cases. Perhaps we should call this special kind of prediction "historical prediction."

Frederick Teggart himself pushed far afield in using correlations as the basis for constructing a science of history. He was interested in the Roman Empire and the barbarian tribes on its borders. Since he wished to show that history could be organized as a science, as well as narrated as a branch of literature, he summed up his study not in a narrative but in a proposition of correlation, and marshalled his evidence with as little narrative as possible. He stated the correlation thus: "Within these decades [58 B.C. to A.D. 107) every barbarian uprising in Europe followed the outbreak of war either on the eastern frontiers of the Roman empire or in the 'Western regions' of the Chinese."[10] The significance of Professor

[7] Teggart, Frederick J., The circumstance or the substance of history, *Amer. Hist. Rev.*, **15**: 715, 1910.

[8] Berkeley, Univ. of Calif. Press, 1939.

[9] Consequently archeology might be called an experimental science. *Cf.* Albright, William Foxwell, *From the Stone Age to Christianity,* 24, Baltimore, Johns Hopkins Press, 1940.

[10] *Rome and China,* vii. Practically speaking a fatal defect in Teggart's demonstration is his inability to establish that the events correlated—"uprisings" or "wars"—

Teggart's effort for our evaluation of method is in his way of explaining this correlation. He maintained that the wars in the East disrupted trade and so led to the barbarian uprisings in Europe. True to the "scientific" scheme of thought which he was trying to imitate, he looked for general laws about wars, trade, and the effects of the severance of trade. He wished there were such laws, confirmed empirically by other historical correlations in addition to his, for then his explanation could be comparable to the kind of explanation given by a physicist who explains Kepler's laws of planetary motion by referring to Newton's more generalized laws. But in fact Professor Teggart knew no such general laws about trade and war.[11] Instead he has to leave his explanation of the uprisings at the point where he has given us to understand that interruption of trade created barbarian discontent. Thus his need for explanation drove him to make some inferences about the state of mind of the people being studied.[12]

As soon as he began describing action in terms of the aims of the actors and their pattern of behavior as social types, he was no longer dealing simply in propositions of correlation; he was using structural propositions. Under structural propositions I include those of the sort that economists often call models and that Max Weber called ideal types. These models or types are analyses of action which use the concept of an actor with defined aims and methods so that his reaction to various conditions will form a consistent pattern and may be predicted. The actor referred to by the structural type or model may be either an individual or a social group. Professor Teggart's explanation implies a type of tribal society which profits from trade passing through its territory and reacts to the interruption of that trade by attacking neighbors.

I have taken up Professor Teggart's use of structural propositions precisely because of the remoteness of his theme. He did not have a position in the historical profession comparable to G. B. Adams, E. P. Cheyney, or Carl Becker and could not embalm his philosophy in a presidential address before the American Historical Association. From many points of view it seems strange, even bizarre, to test the possibility of making history a science by a theme requiring assertions concerning the behavior of ancient tribes in inner Eurasia of which almost nothing is known directly. But for that very reason Professor Teggart's intellectual adventure demonstrates incisively how the historian needs and creates types or models intelligible in terms of motives, even if there is no direct evidence that their counterparts existed in reality. If Frederick Teggart had chosen to correlate the making and breaking of alliances in modern times with the volume of trade between the countries involved, he would have found his sources full of evidence concerning the motives and social structures connecting alliances and trade. This evidence would have created other complicated problems of methodology. But in the sources for his *Rome and China* he found almost no evidence concerning the process by which interruption in trade led to war. His correlation was empirical, as nearly as he could make it; that is, it was based on the evidence concerning what actually happened on the Roman and Chinese frontiers; but the structural proposition by which he explained the correlation was purely hypothetical. Professor Teggart devoted a lifetime to an experiment in method. The outcome seems to me to show the limitation of trying to explain history by correlations. It illustrates the inevitability of structural propositions in organizing historical knowledge.

A complete contrast to Professor Teggart's work in most every respect is that of the International Committee for Price History, yet the outcome of their investigations seems to me to confirm also the importance of structural propositions compared to mere correlations. These students of price history did not of course start out to collect data purely empirically and see what correlations would emerge, although that has been done to an imposing extent in the study of business cycles. Research in price history was guided very largely by the quantity theory of money. In a recent statement on the uses of price history, Earl J. Hamilton said, "Monetary theories that meet the test of logic and internal consistency are usually verified both by their authors and critics through an appeal to experience, and no phenomena are more illuminating than the behavior of prices over

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are similar one to another in the respects pertinent to the inquiry. See my review in *Amer. Jour. Philol.* **63**: 356–357, 1942.

[11] His searchings for them are reported especially in his *The processes of history*, New Haven, Yale Univ. Press, 1918, and in parts of his *Theory of history*, Yale Univ. Press, 1925.

[12] The state of mind is not usually explicitly referred to, to be sure, but at least once he slips and refers to "suspicion" as the inspiration for an attack—*Rome and China*, 240–241.

long periods of time."[13] He then analyzes a number of examples, to which might be added his own study of Spanish prices. But, as Professor Hamilton pointed out, these studies of correlations between prices and the quantity of money have not all substantiated the same theory. Consequently more and more emphasis is being placed on the mechanisms by which increases in gold, silver, or some other form of money, acted on the price systems.[14] This emphasis on "mechanism" has meaning both to the theoretical economist and to the historian. To the theoretical economist it means more attention to analytical models of aggregates by which to study prices. To the historian, it means more attention to the culture and the institutions of the various countries which received the gold and silver of the New World.

I have drawn one example from the field of history and other from the border at least of economics. Sociology will supply a third. Talcott Parsons in his *Structure of Social Action* analyzed the work of four scholars, partly sociologists partly economists, and concluded that although they started from diverse intellectual backgrounds, the nature of the problems they were trying to solve drove all of them into using basically the same conceptual scheme, the scheme which I am calling structural propositions. Professor Parsons analyzes elaborately what is involved in this approach. Just as there are some concepts suitable in the study of matter—mass, motion, location, and so on—there are other concepts suitable to the study of human relations. The essential in the analysis of action are an actor, his aims or purposes, the conditions he faces, the means he uses, and the normative standard by which he relates the means to the end he has in view. The "actor" may be a person or a society.[15]

This conception can be expressed in various ways. It is in Robert M. MacIver's description of "dynamic assessment."[16] It was implicit in that part of Marx's system which was concerned with the *process* of change. It is dramatized in Arnold Toynbee's formula or challenge and response.[17] They are all examples of what I am now calling structural propositions. They explain the process of change by stating the relations between the different attributes or elements in various types of action.

Having illustrated my theme by pointing to some trends in history, economics, and sociology, I shall not venture further afield. From the conversation of friends who are students of literature and language, I have discovered that they have arguments about positivism which seem to resemble the arguments over the scientific nature of history. But if the large topic assigned me is to be covered, I must appeal to your competent imaginations to apply my suggestion to other areas of the humanities and the social sciences.

But, it may be objected, the test of any system of knowledge is its ability to make predictions. The study of correlations has enabled natural science to predict, and, as I pointed out with reference particularly to archeology, correlations are also the basis for much historical prediction about human relations. Will knowledge organized in structural propositions do as much?

In answer it is to be noted first of all that the kind and amount of actual, accurate prediction about human relations which can be based on correlations alone is extremely limited. For this purpose a distinction must be made between correlative propositions which are empirical, because they sum up our observation of past recurrences, and those that are hypothetical. They become hypothetical as soon as they include the phrase "other things being equal." In that form they cannot be applied directly to the world of human reality, either historical or contemporary, in which "other things" are never unchanged. Empirically observed correlations on the other hand, such as those to which I referred concerning types of pottery, have a relatively very narrow range; or else it becomes very hard to give any operational definition to the elements being correlated so that they can be identified in examining an additional historical event in which their presence is in question.

Structural propositions also are hypothetical or empirical. Those most familiar to the economist, which he calls analytical models, are hypothetical and often highly abstract. An example is the

[13] Hamilton, Earl J., Use and misuse of price history, *The Tasks of Economic History*, supplemental issue of *Jour. Econ. Hist.* **4**: 52, Dec., 1944.

[14] E. g., Schumpeter, Joseph A., Theoretical problems of economic growth, *The Tasks of Economic History*, supplemental issue of *Jour. Econ. Hist.* **7**: 5–6, Dec., 1947.

[15] Parsons, Talcott, *The structure of social action: a study in social theory with special reference to a group of recent European writers*, 731–736 and *passim*, N. Y., McGraw-Hill, 1937.

[16] MacIver, Robert, *Social causation*, chap. XI, N. Y., Ginn., 1942.

[17] Toynbee, Arnold J., *A study of history* I: 271–299 and less mythologically on 299–301, Oxford Univ. Press, 1935.

statement that the entrepreneur combines the factors of production so that the last dollar spent on each factor yields approximately the same return. An example more familiar to historians but of essentially the same analytical sort is Machiavelli's characterization of the power-seeking prince. At the other extreme there are structural propositions which are purely empirical, but in that case they are quite specific and refer to particular acts of individuals or social groups. Examples are Machiavelli's statements describing the deeds of Caesar Borgia in language which characterizes these acts as having the same attributes, related in the same pattern, as the acts of an ideal power-seeking prince. The theorist is more interested in the analytical model itself; the historian or any practitioner is more interested in the extent to which particular people or societies can be fully described in terms of one or another group of models. But theorist and historian have some common ground because they are both using the same kind of propositions, and in that sense "talk the same language."

The advantage of the structural propositions in regard to prediction is the way they allow for the uncertainties. The proposition which says: "In so far as the behavior being observed is that of such and such a type—a power-seeking prince or a profit-seeking entrepreneur—his actions will be such and such" invite considering what other models may apply to other aspects of the situation.

At least that is an aspect that appeals to the historian. I am not sure whether it appeals equally to the economist. A historian of the reign of Louis XI of France, for example, would investigate how far the people of that time acted like the princely type, how far like the feudal type, how far like "economic man," how far like craftsmen, how far like obedient Christians, and so on. I can put the same thing differently by saying that he is seeking to describe all sides of the life of the time. To do that at all well he must in any case use imagination and intelligence. The weakness of the historian usually is that he does not have clearly enough in mind the types, especially the social models, which have been developed by the social sciences through analysis and study of the present. Therefore he misses many clues. Especially the historian who insists that history is primarily literature is likely to be mentally equipped only with models that are old-fashioned and mainly political.[18] But the "new history" has done much to remedy that, and the historian recognizes that it is his function to seek to describe all the factors in the situation, and not to apply to the community as a whole conclusions drawn from one element only of the complex of forces shaping it. He is not exclusively concerned with any one model.

The historian's interest in applying several different models to the analysis of each situation he studies is part and parcel of his interest in the kind of proposition first mentioned, as the historian's main concern, the developmental. Although the sweeping developmental laws of history, both those of Edward Cheyney and those of Karl Marx, are on the whole in disrepute, no historian doubts the truth of what Cheyney called his second law, the law of mutability, of the impermanence of nations. This impermanence applies to the varieties of human behavior—feudalism, nationalism, democracy, and so on—which we try to understand by use of structural propositions. For one time one type, such as the feudal noble, for another time another type, such as Machiavelli's prince, is the more pertinent frame of reference for the historian of Italy. As time passes actual behavior becomes more and more like one type and less and less like another type, which finally ceases altogether to be relevant and disappears, as the feudal nobel has disappeared. Analysis in terms of only one model would not be history, for it would not permit analysis of developmental change.

Social scientists studying the present have occasion also to use several models at once and to analyze development. Is it not necessary that they should do so in order to make the best use of correlations? The predictions which are soundest and most restrained are based not simply on the brute facts of correlation, as for example between changes in prices and in quantity of money, but also on knowledge of the process which created a relation between them; for study of the process

[18] In his presidential address to the American Historical Society, at the one hundred and fiftieth anniversary of the Constitution of the United States, Guy Stanton Ford said, "Awareness of what is being done in the other social sciences is a special obligation of him who would write the history of American democracy under a constitution conceived and adopted by an essentially aggressive pioneering people on the eve of an age of science and technology which has carried us farther from them than they were from the Greeks and Romans." Some suggestions to American historians, *Amer. Hist. Rev.* **43**: 268, 1938.

gives some understanding of the purposes and forms of action without which the correlation would be very unlikely to occur. In other words, a correlation holds true only within a certain institutional framework. Study of the structure of action discovers what institutions are so essential that their absence would make recurrence of the correlation unlikely. Then it becomes pertinent to study the contemporary transition from one ininstitution to another. Since the forms of social behavior are impermanent, the social scientist who has completed a voluminous record of correlations and is ready to hazard a prediction may find, when his statistics are all correlated, that his prediction has become historical, comparable in that respect to Frederick Teggart's correlation between wars in Asia and uprisings on the European frontier of the Roman Empire.

The purpose of my discussion has been to show the interlacing of the methods of various disciplines, and I have therefore been unable to restrict myself to a well-ordered vocabulary drawn from one discipline alone. Perforce I have had to yank words around and set them in strange company without taking the time which would be required for formal introductions and explanations. What I have tried to say would have been said by another student in different language in accordance with whatever was his training and background. The three kinds of propositions which I have called correlative, developmental, and structural are all useful parts of our knowledge of human relations. I have tried to suggest something about their relations to each other, and about the way the social sciences and the humanities meet in studying social types developmentally.

PERSONALITY DEVELOPMENT IN CONTEMPORARY CULTURE

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(*Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations*)

THE problem of relating culture and personality is essentially one of defining the variables and measuring the constants of human motivation and social learning. Although the practical research tasks implied by this statement are both great and complex, the logical framework within which the work must be done is entirely unambiguous.

The individual is the molar unit of his society. During his lifetime, he develops motivational systems the action components of which enable him to interact with others. Certain aspects of his behavior are unique to him; the particular objects that call forth his love and hate, his language and gestural mannerisms, the strength of his affiliations and the style of his living differentiate him from his fellows. Other aspects, however, are stereotyped and virtually universal among the members of his society; these include such things as role-playing in institutional settings and the use of common language forms in naming family relationships. Whether the individual's behavior appears to be unique or common depends in some degree on the molecularity of description.

The complexity of these motivational systems is almost limitless. Some of them involve no more than appetites for certain foods or, in adults, routinized methods of maintaining cleanliness. Others involve elaborate interactions with other people. When these require the adoption of different roles by different people, and when the ensuing interactions solve such problems as the exchange of material goods or the rearing of children, the total pattern of societal behavior is described as an institution. The individual also learns to formulate rules and principles to guide his own behavior; that is, he verbalizes what he does and what goal he must reach. Each society possesses a set of more or less official formulations about the customary, and proper, way of solving some of its living problems. These formulations are the culture.

These various abstractions must not blind us to the fact that the ultimate operational referent of social science is the behavior of individual persons. Such terms as *class, family, institution,* and *status position* can be systematically commensurate with terms relating to an individual's behavior only if they are defined as forms of behavior and if the relevant motivational structures are fully described. Every criterion of class membership, for example, is a behavior item that exists in some person's repertory of actions. It may involve a linguistic form, a food preference, a method of rearing children, a way of dressing or the purchase of a kind of furniture. However nonbehavioral the immediate description of the criterion item may be (e. g., "owns a suburban home"), the fact is ultimately a behavior fact.

Socially relevant behavior is based on motives that are gradually built up through social experiences. These systems exist within the individual himself. They are constituted of drives to secure gratification through the making of consummatory or goal responses, and of action patterns that are primarily instrumental in the sense that they enable the person to get in such context with his environment as to permit him to secure the gratification.

These instrumental responses compose the great bulk of what is commonly called culture-conforming behavior. They are of significance because they not only provide the manipulative matrix within which other persons' behavior must occur, but by virtue of the learning process become secondary goal responses themselves. That is, they provide gratification when they occur and the individual will seek to manipulate his environment in order to permit them to occur.

When a man marries one wife at a time in the United States, for example, he is conforming to the marriage rules of a monogamous society. It is not only the verbal rules that make him conform, but a combination of antecedents that includes learned motivation as well. The rule gives him guidance as to what he must do, but the very word *must* suggests the sanctions that exist for the support or enforcement of the rule. These sanctions are by no means limited to immediate threats of punishment for bigamy; in the healthy man who is thoroughly assimilated in his culture, they also include secondary drives that can be

gratified only by his behaving monogamously. Breaking the rules would lead to difficulties of adjustment with other persons who do conform, to be sure, but more importantly, it would frustrate motivational systems inherent in the man himself. He would fail to secure the gratifications he anticipated from the marital relationship and would suffer positive anxieties and frustrations as well.

The fact that adult culture-conforming behavior is dependent on the existence of appropriate supporting motives makes child development a key area for the construction of a systematic social science. As will be seen, the nature of these motives, the kind of behavior which provides him with gratification, depends on the kind of environment he has in childhood. This means that the personalities of his parents are a crucial element. What they do to reward or punish his efforts at social interaction determines the kinds of social interaction that will be desirable or undesirable, in his view, in his later life. In turn, what he does to and with other people in later life will determine not only his adjustment to his society but the extent to which he will stabilize or modify his society's culture patterns.

ORIGINS OF MOTIVES

A newborn infant has various biological needs that have associated with them primary drives. He needs food, warmth, and an opportunity to eliminate waste products. He is so constructed as to have internal stimulation that persists until these needs are satisfied. When he performs the appropriate acts, such as eating or shivering or defecating, to reduce these needs, there is a reduction also in the internal stimulation. Performance of such acts, or goal responses, also strengthens the connection between the stimulus conditions that existed at that moment and the various acts he performed just preceding the goal response. This is a statement of the principle of reinforcement.

The reinforced acts are in most instances instrumental to securing the necessary conditions for further reinforcement. Or if they are not instrumental at first, they become so in either one of two ways. First, the useless acts drop out because on the occasions on which they do not occur the goal response eventually occurs anyway. Second, the adults in the child's environment gradually learn to recognize the child's behavior as having a signal quality. For example, a child is likely to cry when it is hungry. It may also twist its left foot sideways by a species of physiological chance. There is small likelihood, however, that every time he gets fed he will have just twisted his foot. This movement therefore suffers extinction and does not enter into the eventual instrumental act sequence by which food is obtained.

The cry is another matter, however. The mother soon learns to recognize this as a signal that the child wants feeding. Later on in his life, the youngster will develop a whole host of signal acts to control the people around him. His primitive crying response may be reflex in character, and its ease of becoming a signal for feeding may be because it is painful to the mother. This will not be true of language habits; there is nothing intrinsically painful about the words "I'm hungry, mommy!" nor do they become a part of the child's repertory without great effort, but the words are none the less effective as controllers of behavior (Miller and Dollard, 1941).

Another consequence of the reinforcement process is that the instrumental acts become secondary or learned goal responses. In figure 1 the various parts of a behavior sequence are indicated. The drive stimuli that accompany the existence of a primary biological need are indicated by S_D. Other simultaneous stimulation that impinges on the child's sense organs is shown as S_1; this includes lights and sounds, odors and tastes, proprioceptive stimuli, and central symbolic processes. When the appropriate instrumental activity is carried out, exogenous agencies (in this discourse—people) do something and create new environmental conditions, or events, that permit the child to make a goal response (R_G). The instrumental acts gradually become goal responses in their own right after a large number of successful completions of the behavior sequence. Furthermore, the objects that create the favorable environmental event become secondary rewards.

This process has been clearly exemplified in a study by Wolfe (1936), who gave young chimpanzees an opportunity to get food from a vending machine by inserting poker chips in a slot. Although the animals showed no interest in the chips initially, the constant use of them instrumentally in the eating sequence made them desired objects. After the animals had become thoroughly accustomed to their use, they would work for the chips and would hoard them when the vending machine was not available. Later studies have

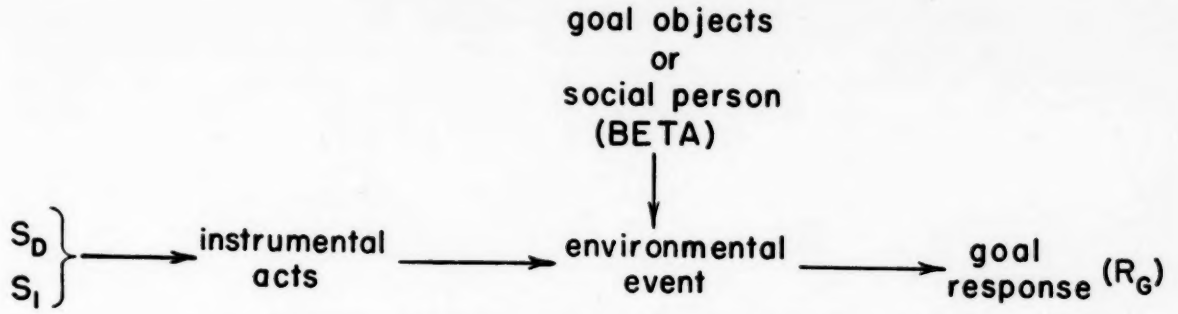

FIG. 1. Instigation-action sequence. This is the basic unit of individual behavior.

shown that such secondary rewards may be used for motivating learning.

Since the instrumental activities are designed to manipulate the environment, the nature of the environment becomes an important determiner of the kinds of activities that will be learned. Likewise, the new motivational systems will be composed of secondary goal responses that require certain environmental events for their occurrence. Hence, the characteristics of the behavior of people who surround the child become the primary determiners of the kinds of motives he will develop.

THE SOCIAL PERSON

This situation is schematized in figure 1. Both the goal object and the social person who collaborates with the child in the interaction sequence are indicated as entering the sequence at the point where the new environmental event is created. The full implications of this social relationship are better exhibited by figure 2. Alpha is the child in this instance and beta is the parental adult. At a given moment each has certain basic drives (S_D) and non-drive stimuli (S_1) operating. Let us suppose the child is hungry and the mother has a nurturant drive. The child cries; this stimulation is part of the S_1 impinging on beta, the mother. The cry is one of the child's instrumental acts. The mother then performs an instrumental act that will lead to satisfaction for her nurturant drive; she picks the child up, holds him against her breast, smiles at him and talks softly as she presents the breast to him. Both of them thereupon have the necessary environmental event for performing the goal response. The child eats (R_G, a goal response to a primary drive), and the mother nurtures her child (Rg, a goal response to a probably secondary drive). The instrumental activities of each have been required for the production of the environmental events needed by the other. This situation describes the fundamental unit of social interaction, with reference not only to ongoing social behavior but also to the formation of motivational systems involving interdependence between people.

Schematic diagrams like figure 2 are so simplified as to rob the process of its human meaningfulness, however. Emphasis must be given to the kind of behavior performed by beta. Mothers do not merely feed their children; they talk to them and snuggle them, they pick them up frequently or rarely, they are tense or relaxed. When such major problems of socialization as weaning, toilet training, cleanliness and orderliness training, and the control of emotional dependency and aggres-

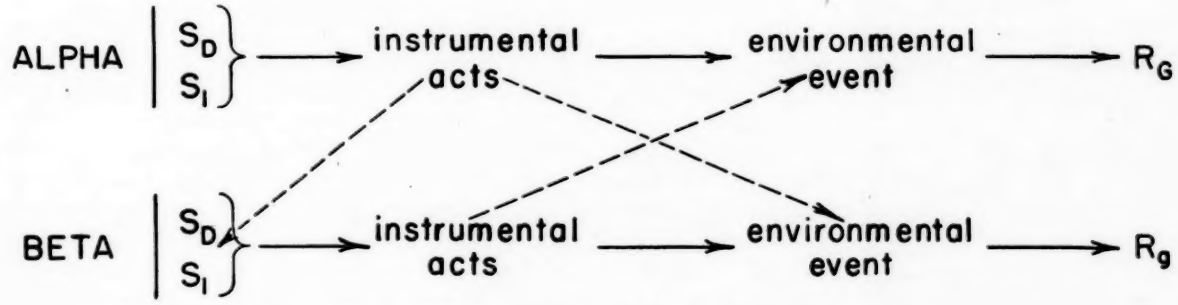

FIG. 2. Instrumental interaction sequence. This is the basic unit of social behavior.

sion present themselves, there are innumerable ways in which parents can vary from one another, and at the same time be consistent in the treatment of their own children.

It is to these finer and yet very pervasive dimensions of behavior that we must look for the origin of individual differences in motivational systems. While the gross outlines of social action are determined by parental reflection of cultural standards, there are many aspects of their behavior toward their children that derive from non-culturally determined characteristics of their own motivational systems. For example, it is commonly said that middle-class American culture puts a high premium on orderliness and the care of material household goods. In comparison with lower class standards this may be so. But within the group of middle class people there are great variations in the style of orderly behavior and great differences in the relative importance of orderliness. To some women, a toy-strewn living room floor is but another chore; if the day is over and the children are in bed, the floor will be cleaned up, as an instrumental act, in order that adults will not stumble and that the next day may start fresh. To other women, the situation is distressing, offensive. It creates discomforting anxiety and represents a threat to the neatness status of the household, possibly a threat to social status. Orderliness has high value in its own right. These contrasting kinds of mothers inevitably treat their children differently. The first ignore messy play, permit children great latitude in manipulative activities within the house, reinforce within-the-house motility. The other group are restrictive and inhibitive with reference to children's play, more frequently commanding, demand low within-the-house motility and reduce manipulative play to a high extent. Because children enjoy exploration and manipulation, the orderly mothers will be more often frustrated by their children and will have a tenser and more hostile attitude toward them; they will set more rigid standards and be more punishing.

These differences between parents can be understood as influences in child training only if they are introduced into the learning situation presented by figure 2. Each mannerism, each approach or withdrawal, each facial expression, laughter and frowning, each command or plea, is a quality of the social person, beta, in the interaction sequence. These qualities, to the extent that they occur with reasonable consistency in the parental interaction with the child, become essential properties of the goal object or social person around whose behavior the child's secondary motives are developed. He becomes a person who is accustomed to orderliness or its lack, hostility or friendliness, much or little help. More importantly, he is motivated to modify his environment to make it like that which he has known if its properties have been associated with reward. And he will be motivated to rebel if there has been too much associated hostility and punishment.

To put the matter briefly, what the parents do and how they do it determines the content of the motivational systems of the child.

THE FIELD FOR RESEARCH

In spite of its empirical complexity, the field for research can be summed up simply. A society has verbal formulations of the way it solves many of its problems, both material and social. The people who compose the society behave with varying degrees of conformity to these formulations. Children reared in a particular culture learn not only the verbal formulations but the necessary motivational systems that will ensure their conforming to the culture. But the kinds of behavior required by the society's adults determine in some degree the kinds of training used for the children. And the kind of people the adults are, defined in terms of their behavior toward their children, determines the kind of motivational systems developed by the children.

This formulation of the relationship between culture and personality development rests on the process of social learning. It requires that systematic analysis be made of the conditions of learning and of the nature of the behavior learned. Such an approach is compounded of ethnography and a detailed psychological study of parent-child interaction. The former is essential for securing a description of the culture, and the latter for discovering how the methods used for socializing the child create the qualities of personality in him that generally characterize members of his culture and specifically characterize him.

The curent research program at the Iowa Child Welfare Station is organized around one segment of this dual problem. It would be impractical to try to secure measures of large areas of contemporary American culture at one time, and equally confusing to try to take into consideration a large number of variables in the personalities of children. Therefore, we are limiting our investigation

at the start to a few highly significant factors that gain their importance from their universality.; they represent problems of social adjustment to all societies.

Two of these relate to biological differences among people that each society must find a way of coping with; these are the fact of there being two sexes and the fact that children have different ordinal positions in a given family. The roles of the two sexes are differentiated by all societies, but there are sharp differences among cultures as to the responsibilities allocated to each, and corresponding to these role differences are motivational differences. Personality characteristics such as aggression have been shown to differ sharply, also; in our own society these differences can be detected as early as age three (Sears, Pintler, and Sears, 1946).

The question as to what learning situations and parental training techniques are responsible for these differential personality developments is by no means a purely academic one, although the study of sex typing should prove an excellent place to discover the basic principles of certain types of motivational development. From a practical standpoint, the degree to which a given child fits his or her sex role is an important determiner of the success of later marital adjustment, a point of crucial importance for the child and a significant variable in its effect on the institutionalized culture pattern of the family.

The significance of ordinal position is greater in some societies than others, because of the formulations that surround primogeniture. Even in our own society, however, which has little formalized role provided for the various ordinal positions, rather startling and consistent differences have been observed between the personality characteristics of first and second children (Dean, 1947). The fact that different ordinal positions provide well-defined differences in the in-family social learning situation makes this dimension a useful one for studying the effects of variation in parental treatment.

Two other universal problems, inherent in the nature of man's societal living, derive from biological similarities rather than differences. It is evident from the analysis of social interaction schematized in figure 2 that the social person, beta, can either help or hinder alpha, the individual whose behavior we observe. If he hinders, alpha is described as frustrated. In order to enforce his demands for assistance, or at least non-interference, alpha can make use of the fact that all people can suffer pain or injury and that they withdraw from the injuring stimulus. Since there are certain vocal and manneristic expressions associated with this withdrawal, expressions that are part of the reaction to pain, alpha has an opportunity to develop a secondary drive to produce injury that will be satisfied by perceiving such evidences of pain. This is the drive of aggression (Sears, 1946). Aggressive behavior in one form or another is found in all societies (Dollard *et al.*, 1939), and all societies develop procedures for controlling it. To some extent these different aggressions, counter aggressions, and controls are formulated by the society's members, but formulated or not most societies, and particularly our own, have difficulties in providing appropriate aggression training for their children.

The other problem posed by the interaction sequence of figure 2 is that of dependency. The care of children during their early years involves repeated contact between parent and child, and many of these contacts are the environmental events required for the satisfaction of primary drives. Hence, the child has an opportunity to develop secondary drives for the satisfaction of which specific kinds of helping responses by others are the secondary rewards or environmental events (Whiting, 1944; Whiting and Child, 1947*a*). As the child grows older, some of these drives must be extinguished if he is to become an independent person able to play the adult role in his society. Recent psychiatric analyses, especially those by Levy (1943), have focused attention on the more extreme forms of the failure to establish adequate independence motivation, but dependency is necessarily a universal consequence of the very nature of child-rearing in any culture. It is the root of affiliative and cooperative behavior, and the problems any society faces have not to do with its total elimination, but with its control and utilization in the maintenance of societal stability.

In our research design, sex groups and ordinal position groups are being handled as independent variables comparable to cultures, while aggression and dependency are considered as dependent variables. The *processes* of sex typing and ordinal position typing are similar to the processes of the development of aggression and dependency drives, and all four represent areas for investigation in which the relationship between culture and personality is intimate. The inculcation of children into the society is largely a problem of training or

rearing for which, in most cultures, the parents are primarily responsible during the first few years of the child's life.

ETHNOGRAPHY

The first step appears to be an ethnographic one. In order to relate the specifics of personality formation to cultural antecedents, there must be an adequate account of what happens to the child. This account must contain several things. First, it must describe the ideals of personality or behavior toward which parents are training their children. Second, it must present the details of how parents formulate the methods by which these ideals are being achieved. And third, it must give a description of both the range and the modality of the actual practices of the parents in their child-rearing.

It would be impossible to cover all aspects of child development in such an ethnographic study; too many ideals would be unique to particular parents or to specific sub-cultures, and there are too many areas of interaction between parent and child to be able to sample them all. As a beginning, the ethnography will be useful if it emphasizes those aspects of the child's behavior that are related to the major primary drives (hunger, sex, sleep and fatigue, elimination), the major secondary drives dependent on social interaction (aggression, dependency, independence, status striving, affection), certain of the routines that play an important part in our own culture (cleanliness, orderliness, hygiene), and reactions to disciplinary control. Parental practices in these areas probably account for the chief influence the parents have on their children's developing motivational systems.

Such data are needed for several purposes. First, they will permit the detailed comparison of various sub-cultures within our own society. Observations by Davis and Havighurst (1946) have shown that lower and middle-class child-rearing practices differ radically in certain respects, and that Negro and white practices are different to some degree even when class position is taken into account. These differences, and many others based on ethnic and religious affiliations, provide the bases for differences in the motivational characteristics of the sub-groups involved. Comparison of ideals of personality will indicate the extent to which there are conflicts within contemporary society and the extent to which specific culture groups are operating out of context with the ideals of contiguous groups. The data will also provide a sample of contemporary American practices for comparison with data on primitive cultures. At present the Navaho, Alorese, Hopi, Kwoma and several Micronesian cultures have been studied with more thoroughness in respect to the basic facts of child training procedures than has the American culture (Whiting and Child, 1947*b*).

Second, child-rearing practices can be matched with the personality characteristics of the children. Data concerning children's behavior are being sought through several procedures, and the ethnographic report of practices is necessary to provide tests of hypotheses relating practices to behavior. The two comparisons having immediate priority are those between the sexes and between ordinal positions within any one culture group. Before antecedent-consequent relations can be evaluated, however, it is first necessary to discover what the parental aims are for their children, and what, if any, differences these differential aims make in the parents' formulations of their child-rearing procedures.

Finally, analysis of the parental formulations of their child-rearing methods will throw light on the relationship between formulation and intracultural consistency.

MEASUREMENT OF PERSONALITY

Two main approaches are being made to the problem of personality measurement in the children. One is on the level of overt behavior and the other on the fantasy level. Various procedures for getting samples of overt behavior are necessary; no one appears to encompass a large enough sphere of the child's life to provide a full picture of his behavior characteristics. One study is in process that involves the repeated observation of aggressive and dependent behavior in a relatively free social situation in the preschool. This work is requiring the definition of these two motivational systems in terms of concrete behavior items that can be isolated and counted through a behavior unit sampling method. Another study is comparing interview data obtained from teachers with performance exhibited by the child in test situations created in the laboratory and designed to facilitate the occurrence of aggressive and dependent behavior. Both studies are oriented around the question of the interrelationships between different manifestations of the dependency drive and of the aggressive drive. A third study is exploring the use of public reputation in a com-

munity as a means of measuring child behavior. Comparisons are being made between ethnic subgroups in communities containing two or more clearly differentiated groups. Judgments about the aggressive, dependent and self-reliant or independent behavior of school children are being obtained from church members, police, teachers, social workers, and others whose public positions require them to deal on an institutional basis with the children under consideration.

Exploratory studies have been completed on another method of measuring overt behavior. Merrill (1946*a*; 1946*b*) has introduced the child and his mother jointly into a laboratory setting and has worked out a method of time sampling for the measurement of methods of maternal control and of types of child response. This method was shown to have a potential source of serious error in it, however, and it is questionable how far one can go with it. Mothers in whom some anxiety about the "goodness" of their child's behavior had been evoked, changed so markedly in their methods of dealing with the child that the obtained sample was unsatisfactory as a measure of everyday behavior. Whether it was satisfactory as a measure of the mother's behavior under conditions of strain remains to be determined.

Still another method, currently being investigated by Dr. Vincent Nowlis, is that of securing data about a child's behavior from an interview with the mother. The interview technique is in part adapted from the methods worked out by Kinsey (1948), but factual emphasis is being laid on the child's reactions to specific instances of disciplinary control exerted in connection with the channeling and control of the primary and secondary drive systems. This procedure will provide not only information about the child's immediate responses to the training situation, but will give a measure of individual differences among the members of a homogeneous culture group with respect to the kinds of disciplinary practices of the parents. This will permit the correlation of the data with other data, both overt and fantasy, obtained from the same children.

Since, from a practical standpoint, the measurement of overt behavior in children is often tedious, expensive, and difficult to manage for many research projects, collateral research is being carried out with projective procedures. Techniques such as doll play permit a study of the child in camera, as it were, and require less equipment, time, and environmental control than do overt behavior measures. Parallel studies on doll play performances are being directed to the measurement of dependency and aggression. The same children who are being observed in the preschool, and whose parents are being interviewed, are being examined in a standardized projective doll play setting by Dr. Pauline S. Sears.

CONCLUSION

This research program is based on the assumptions that the formalized patterns of a society's behavior on the one hand are a product of motivational systems established in its people and on the other partly determine what the adults will do in rearing their children; and that the personalities, or motivational systems, of the children are in turn a function of the society's child-rearing practices. The methods of research being used have been chosen for their effectiveness in providing a measure of the conditions under which children's motivational systems are learned, the process by which the learning is reinforced by parents, and the products of the learning in the children.

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SOCIAL COMMUNICATION

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(Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations)

COMMUNICATION as an *art* has had a very long history. The writer, the orator, the public relations counsellor, and the advertiser have been leading practitioners of this art. Communication as a field of scientific inquiry, on the other hand, has been of fairly recent origin. Within the last decade or so, however, there has developed the promise of a genuine science of communication—a systematic attempt to formulate in rigorous fashion the principles by which information is transmitted and opinions and attitudes formed.

The development of this new field has been at least in part a response to the growing urgency of the problem. In industry the increasing concentration of control has widened the gap between workers and management and the feeling has arisen on both sides of the need for more effective intercommunications. The different frames of reference of management and of the worker have intensified this problem. The next speaker on the program will discuss this topic in connection with labor-management studies.

In our national life the same gap between lawmaker and citizen exists. Formal means of intercommunication have helped somewhat. The radio has brought the political leader closer to the voter and the public opinion poll has brought the views of the citizens more closely to the attention of the lawmaker. But there is still felt by many the lack of more effective intercommunication to replace the lost intimacy of the town meeting in the early days of our democracy.

On the international scene the same lack of communication appears. I recently heard one of our important United Nations officials say "If only we could communicate with the Russians." There is here not the problem of physical communication, but of psychological communication, not a language barrier alone but ideological barriers to communication.

The problem of communications is made more challenging by the fact that it is not an area for an isolated specialist. Adequate understanding of the problems of this field depends on a wider variety of talent and range of specialization than almost any other problem in the social sciences. A real science of communication will require the cooperation of both the practitioner and the scientist. Thus the newspaper editor, the radio broadcaster, the movie producer, as well as the psychologist, the sociologist, the anthropologist, and the political scientist have important roles to perform.[1]

Numerous definitions of the term "communication" have been given but for purposes of the present discussion I should like to define communication as the process by which an individual (the communicator) transmits stimuli (usually verbal symbols) to modify the behavior of other individuals (communicatees).

This definition thus defines the research task as being the analysis of four factors: (1) the *communicator* who transmits the communication; (2) the *stimuli* transmitted by the communicator; (3) the *individuals* who respond to the communication; (4) the *responses* made to the communication by the communicatee. In addition, we must analyze the *laws* and *principles* relating the above elements.

Numerous studies have been made of the communicator and how his characteristics affect the response of those receiving the communication.[2] In this category belong such important problems as the effect of communications in which the true communicator is not revealed, and the effectiveness of appeals made by the communicator in person compared with those in which the message is transmitted through radio, motion pictures or other media.

Analysis of the second factor, the stimuli, has been the most thoroughly studied. In fact when mention is made of communication analysis, or of institutes of communication, which are springing up on all sides, this is the aspect usually meant.

[1] This is well illustrated in the diversity of contributors to the recent symposium: Communication and social action, Yeager, W. H., and W. E. Utterback, ed., *Annals Amer. Acad. Pol. and Soc. Sci.* **250**, 1947.

[2] *Cf.* e. g., Smith, B. L., The political communication specialist of our times, pp. 31–73, in Smith, B. L., H. D. Lasswell, and R. D. Casey, *Propaganda, communication, and public opinion,* Princeton Univ. Press, 1946.

With the growing complexity of our civilization just knowing what material is being transmitted is a gigantic and important task. Analysis of the material transmitted through the various channels of communication has required the development of precise quantitative techniques. The study of the stimuli transmitted by the communicator employs the familiar technique of *content analysis*. Just before the war and during the war work on this problem made rapid strides. The studies of Lasswell, Kris, and Speier on analysis of enemy propaganda through newspapers, radio, and movies constitute good examples of the developments of the last few years in this area. These methods provide analysis in terms of the subject matter of a communication, its thematic content, type of symbolization, kinds of rhetorical devices used, syntactical characteristics, etc.[3] Without the thorough description made possible by these new and more precise methods of describing the content aspect of the stimuli the task of formulating principles and generalizations would be most difficult.

Because the stimuli used in communication are primarily verbal symbols it is important to understand the role of language in communication. Recent developments in the analysis of language and the field of semantics promise important application to the science of communication.[4] The analysis of this problem requires taking into account not only the differences between the languages of different nations but the equally critical problem of the differences in language between groups within our own society—e. g., between the scientist and the layman or between labor and management.

Scientific studies of the material transmitted try to be objective and uninterested in the *values* of the material transmitted. So it is at this point that research must be supplemented by studies of other groups—of the regulation of communication, the maintenance of a free press, and the variety of topics surveyed by the Commission on Freedom of the Press.[5] Here the philosopher, the student of government, and the lawyer, all have a significant place. Research can provide them with objective data basic to their analysis particularly in connection with evaluating the effect of various communication policies.

We now turn to the third of our problems—the analysis of the individual who receives the communication. Here we have the core of the problem of individual psychology. What are the motives of the individuals, what are their capacities, how do their predispositions influence the way in which they react to various stimuli presented? While psychology is most concerned with these problems, other adjacent disciplines have made significant contributions to our understanding. Psychiatry and psychoanalysis have contributed to the analysis of the complex motives of the individual. Anthropological research in our own culture has shown us how the dominant motives and patterns of an individual can be predicted from factors such as his occupation and social class. Thus important information about the individual to be affected by the communication is furnished by census-type data. Just knowing that an individual is twenty-one years of age, wealthy, and has a high intelligence test score permits us to make highly significant predictions of the individual's motives, habits, and capacity to learn, which are extremely relevant to the type of communication used. Procedures derived from clinical psychology and first-hand knowledge of the individual permit still better prediction. This field appears very promising for future developments in our understanding of communication. Studies of other cultures and other national patterns is an integral part of this problem. Better understanding of the countries of Eastern Europe, for example, and consequent better communication with them requires extensive research on the predispositions of individual in various cultures of the world. Work on this problem is now being begun at a number of centers.

The fourth facet of analysis is that of responses to communication. Some aspects of this problem are much more developed than others. One of the simplest responses is that of attention to the communication. Studies of what readers have noticed in communication and what they have read, for example, in the daily newspapers, are relatively numerous. Within the last few years there have been increasingly frequent attempts to relate simple responses, like reading behavior, to the characteristics of the individual responding. Such studies relate readership to sex, educational level, age group, etc.

Similarly a great deal of research on the response

[3] Lasswell, H. D., Describing the contents of communications, pp. 74–94, in Smith, B. L., H. D. Lasswell, and R. D. Casey, *op. cit.*

[4] *Cf.* e. g., Johnson, W., *People in quandaries; the semantics of personal adjustment*, N. Y., Harper, 1946.

[5] Chafee, Z., *Government and mass communication*, 2 v., Univ. of Chicago Press, 1947.

side has been done in the field of radio. But here the primary emphasis has been on the responses of listening and enjoyment. Devices for recording of responses of like or dislike have been of some assistance in this phase of the problem.[6] Only recently have there been corresponding studies of the effectiveness of radio in influencing opinion and in transmitting information.[7] Much the same has been true of the large amount of research that has gone into the analysis of response to the movies. The emphasis has been on what do people like. This has been the aspect that has had the great commercial backing. The beginnings of interest in other directions were shown in the Payne Fund studies on the effects of the movies on a wide variety of phases of social behavior.[8] The work which we did during the war on the analysis of the effectiveness of the films prepared by the Army to give soldiers the background of the war and our participation in it may also be mentioned.[9] Here the responses studied were in terms of the amount of information received, the opinions which were changed, and the effect of changes in information and opinion on motivation.

The analysis of response to communication has been enormously facilitated by recent improvements in technique. Public opinion methods have made rapid strides as will be seen from the papers presented this afternoon. But these methods do not suffice for the many aspects to be covered. Intensive interview methods are needed on many phases of the problem. For some phases of the problem adaptations of clinical procedures will probably prove essential to understand fully the total impact of communication. Another aspect of the problem requiring much further work is that of relating the different types of responses made to communication. This includes study of how changes in opinion and verbal statement are related to other phases of behavior like social action. This brings in the fascinating problem of how to change the way an individual perceives a problem and how changes in his perception affect his other actions.

Our research task also includes the formulation of principles and laws relating the stimuli, the individual, and the response. Here we are immediately stuck by the wide generality of the problem. We find that the principles needed for an understanding of communication are the very ones needed for an understanding of other aspects of behavior all the way from psychological warfare to individual psychotherapy in a face-to-face situation. We are thus at once benefited by the years of basic work that have gone into analysis of psychological behavior and its change, and at the same time confronted anew in changed form with the many large unsolved problems of human relations.

We are, of course, benefited in the search for principles by the years of experience of practitioners of the communication art. Let us survey briefly these contributions.

The field of education has made significant contributions. On the problems of how to transmit factual information, for example, the work of the last twenty years has been very enlightening. But even more significant problems exist in the field of communication of values and attitudes. Here we are largely in unknown territory, with a strong realization on the part of educators of the magnitude of the problem and its importance but with little dependable information at hand.

Another important source of hypotheses is the work which has been done on discussion groups. From wide practical experience a number of excellent books have been written on how to conduct discussion groups, strategic and tactical procedures, and the like.[10] Few of these recommendations have been put to experimental test but there are contained in these books numerous important ideas which should be evaluated.

Some systematic work directed toward study of discussion groups was done by our research group during the war to determine the type of leadership which is most effective and the effectiveness of various types of presentation. Extensive studies of the effectiveness of group discussion were carried out during the war by the late Kurt Lewin and his associates.[11] Their studies indicated that group discussion followed by a group decision to carry out some particular social action, like eating less white bread, or trying out meat

[6] Hallonquist, T., and E. A. Suchman, Listening to the listener, pp. 265–334, in *Radio research, 1943–1945*, Lazarsfeld, P. F., and F. N. Stanton, ed., N. Y., Duell, Sloan, and Pearce, 1944.

[7] *Cf.*, e. g., Wilson, E. C., The effectiveness of documentary broadcasts, *Public Opin. Quart.* **12**: 19–29, 1948.

[8] Charters, W. W., *Motion pictures and youth: a summary*, N. Y., Macmillan, 1933.

[9] *Experiments on mass communication* (to be published).

[10] *Cf.*, e. g., Elliott, H. S., *The process of group thinking*, N. Y., Association Press, 1928.

[11] Lewin, K., Forces behind food habits and methods of change, pp. 35–65, in The problem of changing food habits, *Bull. Nat. Res. Council*, No. 108, Oct. 1943.

substitutes, was more effective than lectures and individual decision. This research raises a large number of important questions for research as to the factors responsible for the alleged effectiveness of group decision, such as the interelationships between the members of the group, the personality characteristics of those who are and are not influenced by the group pressures, and the like. Such research should greatly clarify our understanding of the interrelationships of the individual and the group in communication. Other significant work is being planned in the analysis of conference discussions and their effectiveness in various situations.

Work on psychotherapy has been of value as a source of hypotheses. This represents an important form of communication—of the face-to-face variety. From the extensive work on this problem many hypotheses have developed which if confirmed should be applicable to mass communication as well. Let me quote a single example. It has long been a belief on the part of many psychotherapists that decisions reached independently by the client or patient are apt to be more influential and lasting than those suggested by the therapist. Some work has been done along this line for individual psychotherapy. How about this generalization at the mass communication level? Is it more effective to present evidence for the point being communicated without drawing the implied conclusion and letting the communicatee draw the conclusion? Or is it better to present the evidence and also draw the conclusion for the reader or hearer? Preliminary work we have underway seems to indicate that an important variable is the intelligence of the individual or group addressed. With the more intelligent members of the audience the effects may be more lasting when they participated in the decision process but with less intelligent members the correct conclusion may not always be seen and grasped without being made explicitly by the communication.

Advertising wisdom yields a number of important hypotheses. Where systematic results are available they have considerable significance because of the fact that the objective of the communication is usually clearly defined. This is in contrast to certain other areas where people are eager to communicate but it is extremely difficult to define what they are trying to communicate so that one can make any adequate evaluation of the success of the effort.

There are a number of important limitations, however, to work in advertising as a source of principles of communication.

The first is the fact that many of the results are kept confidential because of their commercial value. A second difficulty is the complexity of the situation in which research is carried on. Organizations frequently have simultaneous radio, newspaper, magazine, and poster advertising with complex temporal relations which make it difficult to attribute results to specifiable causes. Thirdly, the research has been done primarily without reference to theoretical systematization so that it is difficult to generalize the results to new situations. Results are most frequently of the type that ad *A* produces more sales than ad *B*, but without any systematic account of the respects in which the two ads were the same and in which they were different.

Lastly, important ideas have come from analysis of problems of communication in industry. The importance of this problem was clearly shown by the early studies of Mayo [12] and others. A book like that of Chester Barnard [13] on leadership has a large number of significant hypotheses as to the role of communications, the formal and informal channels which exist within an organization and the barriers to more effective communication up and down the lines of organization and across them. Systematic work is being begun on the problem of communication within management, between management and the worker, and between management and the public.

The problem confronting us currently is not then lack of ideas and hypotheses. These as we have seen are available on every side. What are primarily lacking are two things: (1) lack of a comprehensive theoretical structure to embrace the diverse ideas and hunches from the various fields and (2) systematic experimental work to check and verify or refute the hypotheses obtaining.

In systematizing the field the work which has been done on the psychology of learning has proved of enormous help. This is not unexpected when we stop to consider that communication is essentially that phase of learning in which the conditions for learning are set up by another individual, the communicator. Thus all teaching is communicating.

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[12] Mayo E., *The human problems of an industrial civilization*, N. Y., Macmillan, 1933.

[13] Barnard, C. I. *The functions of the executive*, Harvard Univ. Press, 1938.

As a result of the intimate relationship of communication to learning we have a quite sizeable body of principles already available to guide us in the development of a science of communication. We know the elements which are most relevant to analysis of this process—stimuli, responses, motives, and rewards. We know something about the conditions under which new habits are acquired. We know what happens when the four elements are approprately timed and what happens when they are not.

These principles tie together a lot of the hunches derived from some of the practical groups mentioned earlier. But they also tend to make us skeptical of some of the theories advanced by some communicators. Let me cite one example.

Many advertisers assert that any communication will be effective if it is repeated frequently enough. Psychological theory, on the other hand, stresses the fact that repetition will only be effective when the proper combinations of the other elements of motive, reward, and response are observed. Without a response leading to satisfaction not learning, but "unlearning" should occur. At the superficial level we could say that many repetitions will strengthen the desired behavior but the strengthening is not the result of repetition alone.

This approach of the advertiser to the problem of communication is illustrated in the story in today's *New York Times* concerning a planned campaign to eliminate race prejudice.

> An aggressive national advertising campaign utilizing virtually every type of medium reaching the public, with the objective of "making racial prejudice as unpopular as B.O." will be carried on by the Advertising Council, Inc., it was announced yesterday.
>
> During a press conference prior to the announcement of the campaign, Lee Bristol, vice president of Bristol-Myers and campaign coordinator, said: "Every means of advertising which could probably be useful will be mobilized in this fight against prejudice. Some of the best brains in the advertising profession have donated their time and talent to the design of striking posters and eloquent statements of the American creed. Through business facilities we will use advertising in newspapers, magazines, over the air, in outdoor display panels, and subway car cards."
>
> Discussing the program, Edward Royal, council campaign manager, said it would be pushed strongly in the South. "We know that there is some support in the South already," Mr. Royal asserted. "We intend to hit the South as hard and as often as we can and feel reasonably certain that much of our material will be run in Southern newspapers and other media consistently." [14]

The objective is a worthy one and communication media may have an important role in attacking this problem, but I am sure you will feel from Mr. Wirth's paper later in the program that some important elements of the problem have not been sufficiently taken into account. However, if the copy for the campaign is adequately keyed to important personal motives, repetition will increase the chances of an individual's being exposed to the communication.

I have stressed the role of psychological theory. Equally important is the development of sociological and anthropological theories concerning communications. The way in which communication is transmitted in various types of social structure is an obvious example where the individual and the group are interdependent. Even more significant is the fact that understanding of the reward system for individuals is only possible in the light of the structure of the individual's society, and that sociological and anthropological analyses of what gets rewarded and what gets punished in the society as a whole, and more particularly in the subgroups of the society, has a most important function.

This brief survey has, I hope, shown that within the last decade there have been developed the basic materials needed to support an all-out offensive in developing a science of communication. The improved techniques for the analysis of the content of communication, the recent improvement in methods of studying the behavior elicited by communication, and the availability of both accumulated experience and some systematic theory make it possible to set up more critical observation and experiments. It appears quite likely that we shall witness substantial progress within the next years on this important new frontier.

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[14] *New York Times* for Feb. 6, 1948.

INDUSTRIAL RELATIONS RESEARCH

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(*Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations*)

THE importance of research on the "frontiers of human relations" is recognized by all who are aware of the problems there. The term "frontier" has a peculiarly exciting and challenging ring to American ears. The continuous breaking of new paths to the West and the conquest of virgin territory have created a role in American society whose prestige rating is high. The frontiersman has been glorified in folklore, song, literature, and sober economic and political history. It is natural that those engaged in the painstaking pursuit of truth via the path of research should seek to surround their activities and thought with the atmosphere of adventure and significance attached to the efforts of those who for three centuries engaged in the conquest of the physical frontiers.

There is a sense in which every piece of research which brings to light new facts or gives order and system to known but poorly understood facts is conducted on the frontiers of human knowledge. Few would challenge this meaning of the term. This general connotation is particularly pertinent to research in industrial relations. In this field, any careful and systematic collection of facts is an exploration in a region whose contours and trails are poorly marked. Although it is true that all research is in the nature of frontier activity, I should like to speak of a relatively recent emphasis and approach in industrial relations research to which that definition would seem particularly applicable. Those representing this emphasis and approach are interested primarily in the development of a fundamental theory of human behavior useful in understanding the activity and relations of people, not only in industrial life, but in whatever area of social interaction they are involved. In other words, they are as much concerned to develop a social science as to increase our knowledge of the facts of industrial relations or to suggest solutions for particular problems in the relations among workers, union leaders, and management.

PRESENT KNOWLEDGE

Few students of industrial relations have made or are making this emphasis; but their number is increasing. As one looks over the volumes which have been written on industrial relations, he is impressed by the relatively large portion devoted to the description of this or that tactic, the promotion of this or that practical solution, and the enunciation of social philosophies which supported particular programs of action. Such emphases have characterized the writings not only of the men of action who have been wrestling with the practical problems of planning and policy determination and administration. They have also characterized the commentaries of the academic men who had no immediate functional relationship to the organization of human effort in the process of production and distribution of goods and services. Relatively few of the latter group have focused their research on the attempt to discover cause and effect relations in the determination of the action of those whose practice they were investigating. Of those who did so, still fewer attempted to express and test their generalizations in terms of a more general social science.

The result could have been anticipated. In recent years an increasing number of leaders of management and labor have been seeking for basic principles of human relations by which they might test their tactics and programs of action, and upon which they might build new techniques for teamwork among the participants in their organizations with the same confidence with which mechanical and electrical engineers initiate modifications in technology. They have sought in vain.

The psychologists probably came closest to providing them with immediately usable principles. They found psychologists who had studied the principles of organic behavior by careful and controlled observations and experiment. They found some who had generalized their findings and "applied" them to the explanations of workers' behavior. Some of them contributed much to the development of tests for prospective employees, attitude surveys, techniques for building morale, training of supervisors, and like projects. They found a few who had undertaken a careful analysis of the motives for and determinants of behavior among workers themselves.

These leaders in search of a science found many "human relations consultants" and "human engineers" ready to advise them on the basis of what they declared to be the "sound and tested principles of psychology." Many of these were helpful in the development of personnel programs.

The searchers found anthropologists who had studied primitive tribes and who had useful suggestions about the structure of human organization and about the influence of a role within that organization on individual behavior. Some of these labeling themselves "applied anthropologists" had made stimulating applications of their methods and concepts to contemporary social groups including those in industry.

They found sociologists who had studied criminals, relief clients, church members, divorcees, strikers, minority groups, farmers, unemployed workers, politicians, and all manner of social institutions from the family to the League of Nations, and had developed many useful hypotheses mainly about the way human behavior is shaped by its environment, and about the social circumstances which produce friction in relations among people.

They found economists who were ready to explain what they called the behavior of the labor market and the actions of workers and employers on the assumption that a worker acted like a "seller" and the employer like the "buyer" of a commodity.

Facing this mass of conclusions related for the most part to people and acts and social devices only loosely connected with the field of industrial relations, finding no common theory of human behavior running through all of this research in human relations, it is not surprising that they did not find what they were looking for, a dependable and useful science of human relations. It is not surprising that the men of action did not find what the men of science had not been able to find or produce, an integrated body of principles of human behavior by which the techniques for the organization and direction of human beings in purposeful group activity could be tested, and on the basis of which effective techniques could be developed.

SEARCH FOR BASIC PRINCIPLES

It is from no lack of recognition of the value of any type of research in the conquest of the frontier of human relations that I would like to devote special attention to the particular type of industrial relations research in which the development of a scientific theory of human interaction and of the determinants of human behavior is a dominating consideration.

You will note that I did not use the phrase industrial relations in this statement of purpose. The reason is simple. Industrial relations are only one kind of human relations. The principles which govern men's actions and relations there can be no different basically from those which govern their social, their religious, their familial, their educational, their political activity and relations. Those doing research in industrial relations who hold to the purpose I have just stated have taken up their homesteads on that particular spot on the frontier marked "human relations in industry" but they are committed to the same objective as those who may be tilling the soil scientifically in other regions.

I would like to discuss briefly some of the features of the approach of those with this purpose.

The ultimate problem in such research is always defined in terms of the question, "*Why* do men behave as they do?" This does not exclude description of *what* men do and *how* they do it, but such description is planned with hypotheses of cause and effect in mind and designed to test those hypotheses.

This leads to the second characteristic of such research. The research directors have in mind a tentative theory of cause and effect which gives them a guide to the kinds of facts they must look for, and enables them to shape their research questions in a significant way. That is, the questions can be formulated so that the answers will enable them to develop or modify this tentative theory. These tentative theories used by the several scientists are not identical, but they are not dissimilar. Usually they have combined elements from the psychological explanations of human behavior in terms of primary and secondary drive reduction and other aspects of personality development, with elements from anthropological and sociological explanations in terms of the compulsions imposed by and the rewards offered by the culture.

Third, these theories place great stress on the explanation of the behavior of men in groups and teams. That is, their primary concern is with the determinants of group-wide or group-wise behavior. This does not mean that the theories have no relevance to the explanation of individual personality and action, but that the research spotlight

is focused on the principles of teamwork. Not satisfied with the explanation of the behavior of men in groups (say a work group or a union) as the summation of explanations of the behavior of each individual involved, they seek the laws of living and survival and efficiency of the group as such and the compulsions which are consequently placed upon every individual participant. The theories consider the societies in which the individuals participate not only as a conditioning influence on individual action but as a partner with biological factors in the production of secondary and tertiary drives, the reduction of which is an objective of individual action.

Fourth, with the requirements of such theory in mind, the men of whom I speak are developing a diagnostic pattern listing all of the relevant facts which it would be desirable to have in any situation in order to explain observed behavior or to predict the effect of any change in the situation upon adaptive behavior. In view of the fact that the theory which guides the research includes principles suggested by psychology and anthropology and sociology, the facts needed are relevant not only to the nature of the persons and their drives, but also to the nature and structure of their social and natural environment, and the character of their behavior and interactions. It is not possible in every case to record all the facts any more than it is possible for a doctor to make a complete physical diagnosis every time he is called to treat a specific ailment. But with the complete pattern in mind, the director of research is aware of what he has left out and the possible effect of his omissions on the significance of his conclusions.

Fifth, the most courageous and perfection-minded of the directors of such research are attempting to develop a preciseness of definition and efficiency of measurement which will make possible the quantitative handling of their data. The value of this aspiration cannot be rated too highly in spite of the fact that the fluidity and complexity of the data are marked. There is danger in the elimination from the factors considered of those which cannot be readily reduced to mathematical symbols, as any alert but humble economist can testify. There is danger that preciseness of a descriptive vocabulary will be confused with accuracy of observed detail. But those committed to the development of science have no choice but to meet these dangers head on and push forward toward their objective, while insisting that the inadequacy of their measuring devices shall not preclude their attention to any factors which have an observable effect on the determination of men's actions.

Sixth, the collection of these facts is conducted primarily by means of first hand observation of groups of people in action and through questions directed to the people involved. This does not exclude the examination of relevant documents or other written materials, but these are considered a supplementary rather than a primary source of information.

Finally, each investigation is considered as a laboratory test of hypotheses previously developed, making possible the amplification or modification or discarding of these hypotheses. Each study is therefore considered one step in the development of a science and not merely an opportunity to produce interesting information about a particular problem.

Here in summary then are the distinguishing marks of the kind of industrial relations research I have been discussing :

1. Definition of the general problem as one of learning the determinants of human action.
2. Shaping of specific questions by reference to the need for amplifying or modifying a basic theory of cause and effect.
3. Emphasis on group-wide or group-wise behavior and on the group as an organic entity.
4. Utilization of a diagnostic pattern outlining all personal and environmental facts needed for adequate explanations.
5. Attention to definition and measurement of data in quantitative terms.
6. Utilization of field observations as the major source of data.
7. Progressive formulation and testing of hypotheses in the laboratory of industrial and union operations.

OBJECTIVES

I have suggested that this type of research in industrial relations is deserving of the label "frontier activity" primarily because it is seeking to develop systematic principles of cause and effect in an area of behavior in which such principles are not understood; it is seeking to supplement the narrow generalizations based on limited experience in using particular tactics with generalizations based on the systematic attempt to trace observed behavior to its sources.

In another connection I have illustrated the difference between this research and much of the re-

search previously conducted in this area in these terms: those whose purpose I have been describing are convinced that the most pressing present need is for studies not only of personnel policies but of the nature of persons; not only of incentive systems, but of human incentives; not only of collective bargaining procedures, but of the factors that mold the attitudes and actions of the bargainers; not only of bargaining issues, but of the structures of living of the parties which give them varying and conflicting "slants" on the issues; not only of the structure of management and unions, but of the compulsions those structures impose upon the actions and thought of the men who represent them; not only of techniques for organizing enterprises or unions, but of the principles of effective association and group solidarity and teamwork.

It is the hope of many of us that such research may pioneer in another respect. It is obvious that the problem of human behavior with which we are dealing cannot be understood in terms of psychology or any one of the social sciences alone. Is it not possible, therefore, that, in attempting to follow the problem wherever it leads us, and employing whatever concepts and research techniques are relevant, we shall be able to define the problem in such a way and develop concepts and a theoretical framework of such a nature that a major contribution will be made to the foundation for an integrated social and psychological science? Whether or not this result appears possible or attractive to present scholars in these fields, we who are studying industrial relations are forced to work in this direction. It is not a matter of choice alone, but of necessity, for we cannot get results satisfactory to ourselves and applicable to the solution of practical problems by employing the concepts, theories, and methods of any one science.

PRACTICAL PROBLEMS

This type of research confronts many problems, in addition to the scientific problems of the research itself. It is expensive. The facts must be gathered through field work of a comprehensive nature. The personnel who can collect the facts must have a breadth of training and knowledge, preferably in more than one discipline. Since they are partners in the development, rather than in the utilization, of a science, they must be persons of disciplined imagination, mental flexibility, and creative capacity. Such a combination is rare. Moreover, the motivation for young men and women with an eye to promotion in particular departments, on the recommendation of men who are departmentally minded, is weak. Those young people who engage in research which in the minds of many able senior psychologists and social scientists causes them to "spread themselves thin" must have a spark of daring and a goodly supply of courage.

A third problem arises out of the necessity for a partnership of the scientist with the men of action, the leaders of industry and unions, in the collecting of facts and the testing of conclusions. These men are the heads of the laboratories in which we must do our work. They must justify their participation to themselves and to those with whom they share responsibility for the welfare of their organizations. While they may hope for immediate benefit, this rendering of a service desired cannot be the primary objective of the scientist. He cannot promise that his research will pay off at the end of the fiscal year and become the exclusive property of the sponsor. Those practical men must be sought as partners who are motivated primarily not to make a "purchase" of a service, but an "investment" in the development of science. This problem does not discourage me, for in contacts with many of these men of action I have found a deep and sincere interest in this latter objective.

Even when cooperation is obtained, however, many problems remain. There is the necessity for accurate and full reporting of results which is not always easy to reconcile with the requirement of avoiding harm, actual or feared, to the cooperating organizations and individuals. There is the necessity for using a language in describing scientific results which is understandable to men who are not trained in science.

I am not discouraged about this latter problem. I consider it an opportunity, a challenge, and a blessing. It is useful and efficient in analyzing data to develop a terminology which is precisely relevant to one's own data. But in doing so the scientist may shut himself off from communication not only with the men of action who must control the "experiments" in human relations designed to test his hypotheses. He may shut himself off also from his colleagues in the several psychological and social sciences whose understanding of his work is essential if any integration of their concepts and theory is to take place.

VALUE OF THEORY

It is not necessary to justify before the members of this mature and honorable Society with a distinguished reputation as a patron of science the central importance of theory in the advancement of knowledge and its application to the solution of the world's problems. It is perfectly clear that if the work of the chemists and physicists had been shut off in 1900 and reliance had been placed solely on the efforts of practical engineers to apply what was known at the time and what they could pick up from their practical experiences that progress in technology and in quality of product would have been seriously retarded.

It is perfectly clear that, if medical theory at the time this Society was founded had remained static and attention been focused on improving techniques, present resources for improving the chances of a healthy life for the individual would not have existed. And it is difficult to see how, on the basis of that theory of the maldistribution of the blood, any program of public health could ever have developed.

To the engineer and the doctor and to all who profit by their efforts, theory is the most practical thing in the world. So it is for the planners and administrators in the field of industrial relations. And so it is for all of us whose happiness and welfare can be increased if their action is wise, and destroyed if they blunder.

It is a truism to say that the increase in quantity and quality of material goods which flow from industrial operations is as dependent on the effective coordination of the activities of people and their efficient teamwork as upon the adequacy of tools and technology. It is equally obvious that the processes of democratic society are seriously disturbed by industrial warfare. The public interest lies in the increasing flow of goods and in the maintenance of peace. For the practical decisions in ordering human relations so that these ends shall be served, we depend on the leaders of industry and unions. Changes in technology, in economic, social, and political circumstances, and many other factors, are constantly injecting elements into their problem of which the old techniques took no account. He is poorly equipped for adaptive action who knows merely what to do but not *why* he does it. It is the purpose of theory to supply the answer to the question, "Why?"

At this stage in the development of the type of research in industrial relations I have been describing, there is little to report which can appropriately be labeled sound and tested theory. A beginning has been made and effective results have been obtained here and there by utilizing a few principles which for the most part have been adapted from those already well established, chiefly in psychology and anthropology. Rather than attempt any summary of these in a discussion of frontiers in industrial relations research, I have chosen to report the growth in this field of an interest in and effort toward the development of a science of human relations. I have chosen to report the trend toward first hand investigation and analysis of the human behavior and relations to which the developed principles will be applied.

If the promise is realized in results it should be possible in the not too distant future to report not only objectives and the trend of research effort, but scientific findings which can be subjected to the test not only of debate but also of experimental application. When that time arrives, we shall know whether it was profitable to stake out our homesteads on this frontier of human relations. In the meantime most of us can report that life on this frontier is exhilarating and challenging and that we have no nostalgic yearning for the traditional comforts and security of departmentally "civilized" academic life.

RESEARCH IN RACIAL AND CULTURAL RELATIONS

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(Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations)

I

PROGRAMS for the improvement of race relations have enlisted wide popular interest and substantial material support. Hundreds of national, state, and local public and private organizations are at work pursuing activities designed to solve practical problems in this field. It must be admitted, however, that perhaps to a greater degree than in other problem areas, practice far outruns reliable scientific knowledge. The thousands of men and women of good will who are engaged in improving the relations between racial, religious, and cultural groups that comprise America have been guided by moral, religious, or patriotic convictions, by common sense, and by such expert knowledge as chance has made available to them. They trust that what they do makes a difference and leads to improvement; but they are not certain.

Most of the present programs assume that it is possible to change human behavior by teaching, preaching, propaganda, and legislation or by exposing people to new experiences and altering their conditions of life. The assumptions upon which these efforts rest, however, have not been critically and systematically examined. Only the first steps toward testing the validity of the theories underlying practical action have been taken. In view of the acuteness of the problems, their importance for national well-being and the future of America's position in the world, the impatience of the action agencies is understandable. They want practical results and they want the research workers to deliver them now. Some of the more impetuous of the action agencies are either convinced that they already have all of the answers or so sceptical or naive about the ability of social science to give them valid answers to their questions as to be inclined to ignore research altogether.

The ability of the social sciences to furnish answers to questions of policy and techniques in the field of racial and cultural relations is limited by many obstacles. In this field, as in so many other areas of social life, the research worker labors under the handicaps that every layman considers himself an expert and that controlled experimentation is difficult and costly. Competition between agencies is keen and there is a tendency to claim that such beneficent changes as take place are due to one's own program whether or not such claims are justified. The very multiplication of agencies and programs, however, and the more critical attitude of the supporters of these programs have led to some degree of coordination of effort and have given impetus to concerted programs of research both in the action agencies themselves and in the universities and research centers.

II

Progress in the development of more effective techniques for analyzing and treating problems of inter-group relations has been aided by the more general acceptance among reaserch workers of a number of common assumptions, notably the following:

(1) That problems of race relations can be more adequately understood if they are viewed generically, i. e. if Negro-white relations are seen as not fundamentally different from the minority-dominant group relations involving Catholics, Jews, Spanish-Americans, and Japanese-Americans.

(2) That racial and cultural relations are not a realm apart from the rest of social life but manifest themselves in the normal routines of living, such as business and industry, the school, the church, the labor union, the courts, the army, the professions, in recreation, health, housing, welfare, and in community affairs.

(3) That the problems of racial and cultural relations are the product of the loosening of local ties, of widened and intensified human contacts incident to mass migration, mass communication, industrialism, and urbanism and to the world-wide diffusion of the democratic ideal which has inspired the quest for freedom and equality among many peoples who formerly accepted their lot without protest, and hence could not be considered minority peoples.

(4) That race relations are not essentially different from other inter-group relations such as

industrial relations and international relations, and that they must be approached as we approach other interpersonal and intergroup attitudes, prejudices, antipathies, tensions, and conflicts, keeping in mind, of course, the complicating factor of racial and cultural differences.

(5) That racial and cultural relations are affected by the character of the society, its traditions and laws, its resources, its stability, its stratification, its economy and the composition, distribution, and succession of its component population groups, and

(6) That, although there has always been and probably will always be intergroup conflict, not all of this conflict is inevitable, and that, given the will and the intelligence, much of it is subject to prevention and control.

III

Despite the enormous volume and great variety of practical activity in this field there is at present no reliable evidence to show what, if any, contribution this prodigious effort is making to improve the situation. In view of the serious nature of the problems and in the light of the results obtained by scientific study of other fields it would seem, therefore, that if even an infinitesimal proportion of the total resources now devoted to action programs were diverted to the ascertainment of the effectiveness or ineffectiveness of these programs, both the field of race relations and that of social science might be well served.

Lest anyone think that race relations is merely an *ad hoc* area of applied social science or of social engineering, let me say that although I would not be ashamed to admit it, if it were true, such a view, in my opinion, involves a fundamental misunderstanding of the social sciences. Our task is to study social reality, i. e., the life of man in society, and racial and cultural relations are one phase of this life. There is no other realm of existence for us to study, although we can approach this as well as all other phases of social existence on varying levels of generality, abstractness, or remoteness from the scene of action and policy.

There is scarcely an aspect of race relations which social scientists might not study with profit and there is no social science discipline which does not in some manner bear upon the problems of race relations. To arrive at sound social policies we need, to be sure, all the relevant knowledge that the social sciences have to offer, and more; we need wisdom, courage, freedom, and the power to put our knowledge to use. We may not be able to answer all the pressing immediate problems at once, but if we are skillful we can define our subject of inquiry in such a way as to make it both scientifically valuable and practically useful. I know of no rule in science which says that we must halt our investigations whenever they threaten to have important practical consequences. On the other hand there is no rule of practical conduct which dictates that all action must cease until science has given us a complete and final answer to our questions. We know that *all* the facts will never be in, and meanwhile we must act on the basis of such knowledge and wisdom as we have. I for one am willing to start with common sense, for that, contradictory as it may be, is all that we have to start with; and I shall be content if, for some time to come, while we are trying to improve upon the accumulated empirical wisdom of the human race by means of science, we can assure ourselves of achieving an even slightly better than fifty per cent probability of correct answers.

At present, however, the conflagrations reported all about us keep us busy trying to put out fires when we are not at all certain as to whether the hose that we use is filled with water or with gasoline. In discussing his program with a leader of a national organization in the race relations field recently I suggested that the time might have come to evaluate the effectiveness of his program. He was frank enough to tell me that, even if an objective appraisal showed that the money they spent did no good, they might still continue to carry it on because probably it did no harm. Fortunately, however, both for race relations and for social science, there is increasing interest among the technicians and executives of the race relations agencies as well as among the donors of their funds in a critical appraisal of the assumptions upon which they proceed, the methods they use, and the results they achieve. The reception which the program recently initiated at the University of Chicago for Education, Training, and Research in Race Relations under grants from the Rockefeller Foundation and the Carnegie Corporation has received is indicative of this new orientation.

IV

One of the serious obstacles to the improvement in race relations has been the ambiguity of the objectives to be attained. There is a disposition to assume that, if only the people of our community, our nation, and the world were to forget

about the different racial, religious, and cultural groups in their midst and were to treat them all as human beings, all would be well. But the fact is that different racial, ethnic, religious, and cultural groups are a stark reality or at least a fairly rugged product of our imagination and they complicate our lives and trouble our consciences. We shall probably never live in a world without having to take cognizance of these differences, although most humane and enlightened people that I know wish that they could live with others in peace, order, and under freedom retaining their own integrity and recognizing others on the basis of their personal qualities and achievement rather than their race, creed, or national origin. If you happen to live in the southern part of the United States or in South Africa, however, your conception of proper race relations is circumscribed by a deeply rooted etiquette of racial relations which prescribes strict segregation, a belief in white supremacy, and grossly unequal opportunities. Hence many otherwise enlightened Southerners are for the present unable to share our professed ideal of non-segregation and equality of opportunity, to which, let it be said, we ourselves do not always adhere.

This calls attention to the fact that what is a problem of race relations in one area and one culture may not be a problem in another area or culture and that even in any one community the values that are to be striven for are not always unambiguous and mutually compatible. Since social problems arise out of the recognition of a discrepancy between norms and practices, between aspiration and realization, or as Mr. Myrdal [1] has put it, between our creed and our performance, the social scientist seeking to understand these problems cannot afford to ignore the factors that make them problems, namely the value system of the community or society in question.

It is the social scientist's business not merely to ascertain what the dominant values of the society are, but to show their compatibility with each other, the extent to which they are realized or realizable and at what social cost. It will become quite clear in the course of such investigations, whether carried out through historical studies, life histories, community surveys, intensive interviews, or mass attitude and opinion polls, that not all alleged values are actually cherished and that they are not all equally fervently sought. From the standpoint of the strategy of race relations such studies might be crucial for they might reveal, for instance, that what the Southern whites most fear and are most reluctant to concede, namely Negro-white social equality, especially intermarriage, is not what the Negro most wants; and that what the Negro wants most, namely equality before the law, equal job opportunities, and educational opportunities is precisely what the whites would be most likely to yield.

Thus, while the social scientist does not set the values of society, it is his proper role to discover what they are, how they are structured, by whom and how genuinely they are held and what the consequences are of the attempt to realize them upon other values, upon various segments of society, and upon society as a whole. By making the prevailing values explicit the social scientist can use them to discover what society takes for granted and obtain a more reliable view of the tacit assumptions underlying thought, feeling, and action.

V

Important as is the recognition of the values of society for an understanding of the kind of social world toward which a given group of people is striving and for making explicit the generally unstated assumptions upon which people act, this analysis of the structure of values is only an initial step toward the scientific study of race relations. If we are to appraise the effectiveness of programs of action it is essential that we understand the specific objectives which the techniques used are calculated to achieve. It is well to recall that in human social life we encounter a kind of causation with which the natural sciences are never troubled, namely motivation. Hence the analysis of the genesis, the types, and modes of operation of motives becomes one of the essential tasks of the social scientist.

One of the most confusing aspects of the race relations problem has been the difficulty of discerning what the varied programs sought to accomplish. Almost all of the programs now in operation will be found upon examination to have as their implicit or explicit purpose one or more of the following: (1) the reduction of prejudice; (2) the reduction of antipathy; (3) the reduction or elimination of segregation; (4) the reduction or elimination of discrimination; (5) the reduction of intergroup tension or conflict. Once these objectives are stated it becomes clear that they are

[1] Myrdal, Gunnar., *An American dilemma: The negro problem and American democracy.* One vol. edition. N. Y., Harper, 1946.

not the same thing and upon even slight further examination it will be found that they are not necessarily correlated with each other.

VI

Prejudices, which may be favorable or unfavorable, usually the latter, are prejudgments of the members of other groups before we have had personal experiences with representatives of these groups. They are based upon the tendency to categorize objects, including human beings, and result in stereotyped conceptions of others. They are evident in our attitudes and actions toward members of other groups distinguishable by racial or cultural marks when we say and act upon the belief that all Negroes, or all Catholics, or all Jews are so and so. Generally we do not invent these prejudices but imbibe them through tradition and sharing the life experience of our group. Prejudices serve a positive purpose; they organize our lives and enable us to act without reflection. We deplore them when applied to racial, religious, and cultural groups because they violate one of our nominally accepted norms of treating a person on his individual merit, because they circumscribe our contact and inhibit us from knowing and appreciating members of out-groups as persons.

Obviously prejudices, which are judgments before we have had direct experience, differ from antipathies which come after having had experience with the members of another group. Of course, if we start with an initial prejudice we are very likely to end up with antipathy, but conversely it is not true that we will necessarily like the members of another group better because we have known them as individuals. The issue as to whether there is such a human trait as xenophobia or fear of the stranger is as yet undecided, but there is some ground for believing that the stranger may as likely be a romantic object of fascination as an object of fear. Prejudice might well be subject to reduction by contact although this is not necessarily true of antipathy.

VII

Segregation which consists in setting groups apart and limiting their contact is another aspect of and at present a very live issue in race relations. It may be based upon prejudice or antipathy or it may be induced by the tradition of one's community, sanctioned by custom and enforced by law. Whereas prejudices and antipathies may be modifiable by personal or group experience and yield to education, segregation usually is a matter of public policy and is more likely to be subject to correction by a modification of that policy through law in which education and propaganda can, of course, play a decisive role. It would be futile, however, to attempt to eliminate prejudice and antipathy by litigation or legislation.

Discrimination is the differential—usually inferior—treatment of persons by virtue of their membership in a particular racial or cultural group. Segregation in the South, for instance, is not regarded as synonymous with discrimination as the legal provision for "separate but equal" public facilities is designed to indicate, although it is increasingly recognized that "separate" generally and in effect means "unequal" facilities. It should be noted, however, that there are many Southerners who though they believe in segregation are genuinely working for the reduction of discrimination.

Aside from their interest in prejudice, antipathy, segregation, and discrimination many organizations are concerned with the reduction of interracial and intercultural tension. This may range from the attempt to reduce intergroup competition or rivalry or rather transforming group competition into personal competition, to the minimization of intergroup anxieties, recrimination, vilification, overt aggression, and group violence.

VIII

Organized effort in the United States on the part of race relations agencies has correspondingly had a negative and a positive aspect. On the negative side, which has enjoyed the center of attention because of the more dramatic feature of the activities involved, we have seen campaigns to reduce prejudice and antipathy, segregation and discrimination and tension ranging from overcoming misunderstanding to preventing violence. On the positive side there has been the less dramatic but equally important task of integrating minorities—racial, ethnic, religious, and cultural—into the life of our society. The general social atmosphere and the state of the nation and the world have generally dictated the point of emphasis among the problems and accounted for the particular minority which received major attention. Actual or threatened race riots were in large measure responsible for the founding of numerous official and unofficial agencies in recent years. The persecution of the Jews and the spread of anti-Semitism under the aegis of Hitlerism combined with the urgent

need for full production and the mobilization of the nation for war have done much to make the various agencies formerly interested primarily in specific minorities and in specific problems sensitive to the indivisibility of the issue. It remains true, nevertheless, that the specific objectives to be attained and the specific minority groups concerned, as well as the regional and local situation in which the problems to be treated are encountered, argue for a differentiated approach and selective techniques.

These techniques can be only briefly reviewed here. As Mr. Robin Williams [2] has pointed out, the effects of isolated actions or programs are likely to be small, and they may have repercussions which are indirect, far reaching and have boomerang effects; programs of action are likely to have greatest effect when they operate simultaneously on several strategic factors, rather than one or a few; causal inferences must be made with great care and in the light of all the possibly relevant known factors; panaceas definitely will not be found; and finally the search for knowledge concerning racial and cultural relations must be oriented to the total social system.

Obviously we cannot expect to know much more about deliberately affecting race relations than we know about affecting human behavior in general. We do have a considerable fund of tested generalizations, and in this field we must avail ourselves increasingly of the growing body of fundamental knowledge which social science research puts at our disposal. There is much that the social sciences know that is true, but those who work in the field of race relations must learn what valid social science knowledge is relevant to their problems. This calls for well educated social scientists to advise those who make the policies and carry out the programs. It requires also a generation of thoroughly trained professional experts with a social science background in the field of race relations who will know what questions to ask to which social science research will seek the answers.

IX

Meanwhile out of common sense and such scientific knowledge as we have many practical procedures have been fashioned and are in current use.[3] They consist more or less of shot-gun approaches rather than weapons designed to deal specifically with highly differentiated problems. Among them are:

(1) Information, education, and propaganda through all available media addressed to a variety of groups adjusted to their levels of comprehension and interest. This is based upon the tacit assumption that the truth will make men free and that much of our trouble lies in lack of knowledge and understanding of others, an assumption which in the light of experience is at least subject to some qualifications. This technique rests upon the further assumption that through dramatic, ethical, moral, patriotic, religious, and prestige-bearing emotional appeals interests can be aroused, attitudes can be modified, action can be directed, and participation can be enlisted. It is only fair to note that research in this area has thus far produced more knowledge on the size and composition of the audience reached than on the effect obtained.

(2) Facilitation and organization of intergroup contacts in work, recreation, education, housing, politics, art, and institutional life and encouragement of participation in common activities, together with the establishment of social rules and models to discourage segregation and discrimination.

(3) Establishment of organizations to investigate and adjust interracial and intercultural differences and to air grievances, undertake fact-finding studies, surveys and reports, and to reward and publicise instances of commendable action.

(4) Organization of groups and communities regarded as likely sources of tension.

(5) Organization to expose and combat "undemocratic" movements and to counteract their efforts.

(6) Organization to exert pressure upon public and private agencies to prevent or abolish discrimination in employment, housing, public services, in the press, military service, etc.

(7) Organization to insure high levels of employment, good government, and to promote liberal causes on the assumption that a sound, secure society with a minimum of mass grievances promotes consensus and prevents scapegoating.

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[2] Williams, Robin M., Jr., *The reduction of intergroup tensions: A survey of research on problems of ethnic, racial, and religious group relations.* N. Y. Soc. Sci. Res. Coun., 1947.

[3] Rose, Arnold M., *Studies in the reduction of prejudice,* Chicago, Amer. Coun. on Race Relations, 1947.

(8) Programs for the training of public officials such as policemen, teachers, and other public servants in matters of race relations.

(9) Psychotherapy and group therapy to modify the attitudes and action of individuals and groups manifesting anti-minority sentiments or action or regarded as potential sources of infection.

(10) Political pressure, litigation, and legislation designed to secure fair administration, prosecution, or defense as the case may be, insure the proper presentation in the courts and press for desirable legislation.

X

If the appropriateness and effectiveness of these and similar techniques are to be adequately appraised it is manifestly essential that corresponding research facilities be provided to bring the best scientific knowledge to bear upon their operation. It is also hoped that social scientists themselves in cooperation with the technical experts in the field will invent new and better techniques and will aid in the clarification of the ends toward which they are to be directed. This will require a better organization of the research resources than we now have and more effective exchange of information and cooperation between individual workers and teams of workers in different universities and research centers and in different agencies. In this task we hope we at the University of Chicago will be of some aid through our Committee on Education, Training and Research in Race Relations.

SAMPLING IN STUDIES OF OPINIONS, ATTITUDES, AND CONSUMER WANTS

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(*Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations*)

INTRODUCTION

EFFICIENT sampling techniques that have been perfected during the last twenty years provide to social scientists an accurate and economical means of studying human populations (2, 6, 11, 12, 25).[1] While they are applicable to the study of the behavior of an individual human being, such as the observations of a young child over a period of time, their value is demonstrated most effectively in the study of larger groups, such as those who constitute the potential audience of some means of mass communication. The importance of these sampling methods will increase as methods of observation and measurement in social science become more accurate and the analysis more exacting in its requirements for detailed and representative data.

These improvements in sampling have been incorporated into a number of surveys ranging from national studies of income to surveys of radio listening and newspaper and magazine reading and from the analysis of the effects of bombing on the civilian population in England, Germany, and Japan to the measurement of social and psychological factors affecting human fertility. They have been utilized in surveys of opinion, farm operations, prices, retail dealers' inventories, housing vacancies, and employment. The wide range of these past uses is greatly exceeded, however, by the possibilities of obtaining important data for research in the future by means of appropriately designed sampling procedures.

Recognizing the importance of sampling techniques, the Committee on the Measurement of Opinion, Attitudes, and Consumer Wants of the National Research Council and the Social Science Research Council, financed by a grant from the Rockefeller Foundation, has set up a project to review the advances that have been made in sampling procedures, analyze the reliability of different methods when applied under various circumstances, and prepare an integrated expository report for the use of the increasing number of research workers who are concerned with sampling problems.

This project includes an analysis of the principal techniques that have been developed and a clarification of the principles that are involved in each. It includes a number of inquiries to determine how interviewers actually operate in the field while following a given sampling procedure and what consequences result when they deviate in certain directions from the precise procedures set forth in their instructions. It includes a study of the various factors that affect the cost of surveys undertaken by the principal sampling methods and the possibility of further reducing these costs. It is also concerned with the geographic distribution of population and of various kinds of human characteristics and behavior as that distribution affects the efficiency of various sampling methods. The study is being undertaken with the cooperation of many survey research organizations and individual social scientists.

THE PROBLEM OF SAMPLING IN STUDIES OF HUMAN BEHAVIOR

The concern of social scientists with large classes and groups of people is evident from the discussions that have preceded this paper. Problems of communication, industrial relations, race relations, and cultural influences affecting attitudes and opinions all involve the research social scientist in a study of rather large groups of individuals. The cost of examining or studying every individual in these large groups is prohibitive, and consequently the social scientist is obliged to select a smaller number of individuals for intensive research. It is with the problems of making this selection the present paper is primarily concerned. For the purposes of this paper the term sampling will be used to refer to those procedures, whether explicitly formulated or not, that are followed in the selection of the individuals who are to be the subjects of a research inquiry. This problem of selection has been relatively neglected, but increasingly it has been recognized as a serious and important

[1] Numbers in parentheses indicate references at end of article.

phase of the organization of research activities, requiring adequate planning in advance of the launching of a study. It is also coming to be recognized that the implications of the actual sampling operations are fundamental in the analysis, appraisal, and interpretation of the results of any research project. This is true of many fields of natural science, as well as in the social sciences. Each of these major divisions of scientific endeavor has contributed to the others in the development of improved sampling procedures. Both branches of science have benefited greatly from developments in industry, commerce, and technology, and have in turn made contributions to many fields of applied science and practical work.

In planning a research project, a social scientist is faced with two fundamental questions. The first is a question of "How can I make accurate measurements and observations concerning the behavior and characteristics of the persons who are the subjects of my study?" This problem still faces him if the measurements and observations are made by some other person or organization and he merely accepts their results for further analysis in his own studies. The second fundamental question is, "How should I select the persons on whom these measurements and observations will be made?" This is of concern to him even if the selection is made by some other person or organization from whom he obtains the results. His work is likely to be futile and his conclusions invalid if the measurements and observations are made poorly, even though they be made on an adequate selection of subjects. Likewise, his work will be vitiated if the measurements and observations are made on a faulty selection of subjects, even though they are made with a very high degree of accuracy. An adequate solution of the sampling problem, therefore, is essential to, though not sufficient for, the effective performance of research in human relations and human behavior.

SOME EXAMPLES OF INADEQUATE SAMPLING

The consequences of using inadequate sampling procedures are not always evident. In most instances, it is not possible to obtain accurate information with which to test the results of a sampling operation. Subsequent studies may show that the conclusions were in error, but they usually leave unsettled the question of just why they were in error, whether it was the sampling or other phases of the research method. The lack of dependable tests of the adequacy of sampling procedures has been responsible to a very great extent for the continued use of inadequate methods. Resources devoted to research, in many instances, have been wasted for this reason. NcNemar has discussed the general problem in the field of psychological research (13).

At the risk of falling into the error of generalizing from an unrepresentative selection of examples, we will present several instances that show concretely some defects in sampling for which it is possible to find clear-cut examples. Perhaps the most notorious of these was the *Literary Digest* poll. This fairly simple canvass of opinion was conducted by a weekly magazine which sought to present in its columns the attitude of the American people on certain public issues, and especially on the candidates in the presidential elections. The results of its polling of opinion on issues such as prohibition and New Deal policies could hardly be checked by any set of independent and accurate data since there did not exist at the time any other means by which opinion on this subject could be measured accurately. In the case of the presidential elections, however, the results could be checked by the vote at the elections, which the polls preceded. While there are some unanswered questions about trends and changes in opinion between the time that respondents mailed their votes in the poll and the time when the greater portion of them cast their ballots in the elections, the test provided by the election returns was, on the whole, a satisfactory test of the accuracy of the *Literary Digest* sample. The *Digest* conducted its poll by mailing out to some ten million persons a ballot accompanied by a subscription form for the *Literary Digest* (17). The list of persons to whom the ballots were sent was used at successive polls and revised from time to time. More than two million ballots were returned in each poll. In spite of the great size of this sample and its rather close prediction of the presidential election of 1932, it produced a tremendous error in 1936. Apart from the minor party candidates, the ballots showed approximately forty per cent of the sample favoring Roosevelt and sixty per cent favoring Landon. The results of the actual election were almost precisely the reverse: sixty per cent for Roosevelt and forty per cent for Landon. The difference of twenty points in the scale of percentages was much too large to be attributed to the operations of chance or to any last minute shift of opinion. It was clear and dramatic evidence that the method of main-

taining the mailing list and differences in the rate of response by different groups made the group that replied in the *Literary Digest* poll an inadequate sample for its purpose.

Biases similar to those that were revealed by the *Literary Digest* poll have been found in many mail surveys conducted by other organizations. For example, the United States Department of Agriculture has maintained for many years a voluntary reporting system for crop reporters who provide information by mail each month concerning a variety of agricultural facts. Approximately twenty-thousand questionnaires were returned to the Department each month by its regular crop correspondents (18). In April, 1930, these crop correspondents reported an average of 238 farmers and members of their families who were working on farms, per hundred farms covered by their reports. The Census of Agriculture taken at the same time showed only 122 family workers per hundred farms. The respective figures for hired workers were 92 and 45 per hundred farms. In January, 1935, crop correspondents reported 205 family workers per hundred farms, compared with 158 in the 1935 Agriculture Census, and 77 hired workers per hundred farms compared with only 24 in the Census. The Department of Agriculture was aware of this highly selective and distorted aspect of the sample of crop reporters and made adjustments for it in their statistical estimates, but it was not possible to correct completely the severe biases that resulted from this method of sampling. It was known that the crop reporter lists included a relatively heavy proportion of farms that were larger than average, more productive, and more progressive. These characteristics were to be expected in a sample that depended on the voluntary cooperation of farmers to fill out monthly questionnaires about their own farms and neighboring farms. It should be mentioned that the Department of Agriculture undertook a series of scientific experiments and studies aimed at the development of a more satisfactory method of obtaining farm data and that a number of the improvements in sampling procedure that are now available are the results of this research (9, 11).

A somewhat similar but distinct type of bias can be illustrated by several studies that were undertaken about 1925, to measure the average size of the family from which college students come (22). The typical procedure in these studies was to ask students in certain classes in college to fill out questionnaires giving information about the number of children born to their parents and to their grandparents. This procedure was biased because the chance that families of a given size would be included in the sample was about proportional to the number of children in a family of that size that went to college. Similarly, the families into which the parents were born had larger probabilities of appearing in the sample the larger the number of grandchildren they had that went to college, and therefore the larger the number of children born to the grandparents. The result of this relation between the size of families and the probability that they would appear in the sample was to exaggerate the size of families from which students came and to exaggerate even more the size of families from which their parents came. While subsequent writers pointed out a method of making an adjustment for the bias, the effect of the distortions was to make the results of these studies of dubious validity.

Another example of the effect of using inadequate sampling procedures may be found in the experience of the Italian Central Statistical Institute in 1926, when it was decided to withdraw and preserve a sample of the records of the 1921 population census for subsequent research and analysis before the balance of the files was destroyed (4). A sample of approximately fifteen per cent was selected and preserved, with the intention that it should be representative of the entire population in most of the important statistical characteristics. The Central Statistical Institute decided to use as the unit of sampling certain administrative areas somewhat smaller than provinces, taking 29 of these areas out of the 214 into which the whole country was divided. The sample areas were selected by arranging all the areas according to their birth-rates and certain other population characteristics, selecting every tenth area out of the list, and then adding a number of additional areas whose characteristics were such that they would improve the matching or correspondence between the sample and the entire population on a number of the principal population variables. While the sample agreed quite closely with the population with respect to the variables according to which it was selected, it proved to be very unrepresentative on most other characteristics. Thus, for example, it was more than ten per cent in error in representing the natural increase of the population, namely, the difference between the number of births and the number of deaths, although it had been matched

for births and for deaths considered separately. Similarly, for the density of population, it was more than ten per cent in error. Even for the variances of the variables that were used for matching, it was more than ten per cent in error, with the exception of the variances of births and deaths for which it was between five and ten per cent in error. The errors were very much greater in the case of correlations between variables. It was concluded that the sample was unsatisfactory.

The preceding examples represent serious attempts to obtain representative samples and the defects that they exhibit are exceeded by the defects that undoubtedly have been produced again and again in hit-or-miss sampling by less careful workers. While in many instances crude methods of sampling have yielded results at least as accurate as the relatively crude and inaccurate measurements that were made upon them, it is inescapable that, to attain the degree of accuracy that is requisite for dependable results in scientific research, more effective solutions of the sampling problem are necessary.

SOME EXAMPLES OF SAMPLING OF GREATER EFFICIENCY

In the development of social research, an increasing number of surveys have been conducted which are relatively adequate in their sampling methods. The selection of appropriate examples is difficult, particularly because their adequacy can seldom be appraised very effectively for the same reason that the inadequacy of poor sampling is rarely subjected to a direct test. Most samples are undertaken in the absence of exact knowledge about the variables that are being surveyed. Consequently there is little opportunity to make a direct appraisal of their accuracy. Inferences may be drawn from tests of related variables for which accurate data are available, from analysis of the characteristics of the sampling method, and from considerations based on the nature of the material that is being sampled, but these are not entirely satisfactory. However, we will again risk exhibiting the crude sampling procedures we have criticized, and present a number of examples of more nearly adequate procedures.

In the same field as the *Literary Digest*, one can find a number of public opinion polls operating at the present time with fairly high accuracy as tested by the results of elections and some other tests (1, 10). Most of these other tests are to be found in commercial market research surveys in which purchases of a company's product may be estimated from the sample and compared with actual sales figures. Unfortunately, information about such tests is regarded as confidential by the research organizations and their clients, and consequently is not generally available. Of course, this policy of secrecy encourages belief that there are numerous instances in which the tests show samples that are operating quite inaccurately. Outsiders can only guess as to the extent to which this may be true, perhaps giving some weight to the fact that the research organizations continue to do business for the same clients. For the majority of the reports of opinion polls, however, there is no direct test of the accuracy of result. The 1948 election will provide a further opportunity for testing the sampling methods that are commonly employed in these polls. Perhaps it will be a more severe test than the elections of 1940 and 1944 of their ability to avoid the malfunctioning shown in 1936 by the methods that were used in the *Literary Digest* poll. The accuracy that has been exhibited by various opinion polls in the past fifteen years is the more remarkable because they involve samples that range from five hundred to twenty or thirty thousand persons, or from one-fortieth of one per cent to one per cent of the number of returns received by the *Literary Digest* poll. This is a field in which there is considerable difference of opinion concerning the sampling methods that are being used, a controversy to which we will return later in this discussion.

We may mention several surveys that are to some extent analogous to the Italian sampling of the population census. In 1935 a special census was taken in Sweden by summarizing the population registers that are kept by local government officials and then drawing a supplementary sample from the registers to be visited by the interviewers who obtained data not available in the population registers (21). The sample was composed of every fifth household in cities of 20,000 or more, and in their surrounding suburbs, and every fifth rural community. It was possible to compare the sample with the results of the complete census on a number of population characteristics. The sample was found to deviate from the census by less than one per cent in most instances in which a check could be made.

In the American Population Census of 1940 it was arranged that certain special questions should be asked for every twentieth person enumerated (19). The persons who were thus selected for additional questions also provided a sample for the

quick tabulation of the regular questions and made it possible for the Census Bureau to issue a very large number of statistical results six months or more in advance of the complete tabulations. When the complete tabulations were finished they showed that these sample tabulations were accurate to a fraction of one per cent in most instances and only occasionally differed from the results of the completed tabulations by as much as two per cent. A few of the instances in which there were larger deviations were found to be attributable to certain variations in the procedure of coding information rather than in the sampling itself. A very large volume of detailed statistical data, especially about the labor force, families, and fertility was made available from sample tabulations and could not have been completed without excessive expenditures on the basis of the complete census. These data provide a rich source of material for research, especially in the field of population, and they also afford basic data for the design of more specialized studies in a number of fields of human behavior. The sample was less than twice as large as the Italian sample described above and was only a third as large a percentage of the population. It greatly surpassed the Italian sample in the accuracy of the results it provided.

Still another example of adequate sampling procedure is to be found in the Monthly Survey of the Labor Force conducted by the Census Bureau, in which a sample of sixty-eight counties or combinations of counties were selected and within each such area a further sampling operation was conducted to obtain a national sample of approximately 25,000 households. This sample is canvassed monthly to obtain data on the number of workers employed in agricultural and non-agricultural pursuits and the number unemployed, classified by age and sex, hours worked in the preceding week, and, for those who did not work, the reason. A special survey of a somewhat larger sample in April, 1947, furnished data on migration, education, marital status, income, and other subjects by regions of the United States as well as detailed data on population and the labor force in each of 34 metropolitan districts. The principles by which this sample was established are relatively complicated. They represent an advanced stage in the development of efficient methods of sampling based on statistical theory and experience in numerous previous sampling operations. In this monthly survey, in surveys conducted by the Bureau of Agricultural Economics, in surveys of savings and spending conducted by the Michigan Survey Research Center for the Federal Reserve Board, and in agricultural surveys undertaken in England, India, the United States, and elsewhere, extensive use is being made of large scale maps to divide the country into small units which can be selected at random subject to certain controls and assigned to interviewers. This method of sampling is called "area sampling." It has proved to be highly efficient for many types of surveys.

Before we consider further the reasons for the greater accuracy of the more effective methods of sampling in comparison with inadequate methods, we will examine some of the general conditions under which surveys of human populations are conducted and the difficulties that they impose on the selection of the sample.

DIFFICULTIES ENCOUNTERED IN SAMPLING HUMAN POPULATIONS

The complexity and difficulty of sampling human populations arise from a number of circumstances, some of which are themselves the result of certain types of human behavior. One of the most obvious is the existence of profound regional differences, in custom, attitudes, and opinion. For many studies a sample will not be adequate unless it is spread over the country in a way that covers the full range of regional differences. This introduces at once the cost and inconvenience of conducting the work in a number of distant places. A related difficulty arises from the seasonal migration of vacationists and workers. A further difficulty comes from the daily movement of workers from the homes to their places of work, children to school, shoppers to the stores, and people to places of entertainment or to the homes of their friends. These activities make it difficult to find at home, or in any other type of place, a fair sample of the inhabitants of a community. If the same proportion of persons is taken from each kind of place, including the streets and open spaces, then this difficulty may be overcome but this is itself a complicated task and one in which other difficulties remain. These further problems include the reluctance of some persons to be interviewed, the tendency of interviewers to choose persons whom they prefer for any of a variety of reasons not yet well understood, and differences in the accessibility of various kinds of people even within the same house, office, store, or other place.

GENERAL METHODS OF OBTAINING THE SAMPLE

The easiest way of coping with these difficulties, but the most hazardous way, is to call for volunteers and ask them to come to centrally located offices for study. The call may be made more effective by compensating the subjects for their time and travel expense. Various methods of recruiting through friends and relatives who have volunteered can be used to supplement the call. However, since people differ in their responsiveness to such calls because of factors that may be highly correlated with their behavior, especially the kinds of behavior that are being studied, this method ordinarily produces unrepresentative samples.

Another method is to select a sample from a list or directory and conduct the interviews by mail using a questionnaire that the respondent returns. This method works well in some situations and poorly in others. It may produce unrepresentative results if the mailing lists are inadequate and persons who reply differ substantially in important characteristics from those who do not reply. Various methods of follow-up are used to improve the results of this method of sampling.

One more method is to utilize an established system of communication other than the mails, especially one that requires individuals to appear at official stations and provide information. Examples can be found in the war-time systems of rationing and selective service, in the registration of motor-vehicle owners and drivers, and licensing of various kinds. Each such system omits certain parts of the population but these may not be involved in the study. Each may have certain peculiar features that must be examined to determine whether they will have serious effects on the sample.

Finally, there is the method of sending interviewers to find and interview people under various restrictions and instructions affecting their choice of whom they will select. These instructions may go so far as to name or designate otherwise the persons they must interview in each district. Such restrictions necessarily involve more travel and time in seeking out the respondents than a system that leaves the interviewer free to select the persons he finds at home or on the street and willing to be interviewed.

Each method of obtaining a sample of persons, indeed each variation of a method, has some effect on the processes of interviewing or observing. Persons who volunteer, or respond freely to a questionnaire survey, may be expected not only to differ from others in their characteristics but also in the readiness with which they offer information about themselves and in the distortions and errors that inevitably affect personal information. Interviewers who are free to reject an unwilling respondent and go on to one more cooperative will conduct their interviews in a different manner than those who must make every attempt to complete each assigned interview.

The sampling of human populations itself involves problems in the organization of human effort, the performance of interviewers and respondents in the interviewing situation, and the interaction of various parts of the enterprise on other parts.

CHOICE OF SAMPLING METHODS

Confronted with the question of choosing a general method of sampling to obtain individuals for study, a social scientist will recognize that his choice should depend on the objectives of his study to which the sample must contribute, the particular circumstances in which the sampling must be done, and the resources he has at his command. There is no single method that is best for all situations.

Conceivably he could prepare detailed plans for each type of sampling, estimate their costs and other requirements, estimate the accuracy of the results each would yield and so appraise the net contribution toward the objectives of the study that can be expected from each, if it should be chosen. From such a list he could then choose the best plan, or at least a plan adequate for his purposes.

This procedure is too tedious. Moreover, the set of plans that is prepared may probably omit several possible detailed plans that would be very much better than the best of those on the list. Hence we seek general principles for the design of sampling plans and also means for predicting how they will work out in practice. Particularly important is the forecasting of their ultimate effect on the accomplishment of the purposes of the study.

GENERAL PRINCIPLES OF EFFICIENT SAMPLING DESIGN

A previous paper traces the historical developments that led to modern sampling practice and shows the part played in it by methods originated in agricultural experimentation, primarily in Great

Britain, and in mass production manufacturing, primarily in the United States (20). Ingenious procedures from these fields and from pioneer social and economic surveys have been combined in a coherent system of sampling method that is proving to be highly effective in overcoming difficulties involved in surveys of human populations. This system utilizes probability theory as a powerful means of constructing general principles and comparing various procedures. In its applications it requires certain information about the fundamental conditions, and in its fully developed form it is essentially a variety of economic or engineering theory concerned with the combination of resources and technological processes in the production of information.

The first general principle is that the sample should be scattered evenly throughout the population that is being sampled. The distribution of the sample should match that of the population with respect to geographic location and other important characteristics. Such a scattering was accomplished by Bowley in 1912 when he took every twentieth household in Reading by counting down the street lists in the directory. It was also accomplished in the New Haven Unemployment Survey in 1931, the Michigan Census of Population and Unemployment in 1935, the New York Youth Survey in 1935, the 1940 Population Census, and many other studies.

A second general principle is that the sample should be selected by taking a large number of small units, such as farms instead of whole counties or families instead of whole towns or city districts. However, as the units selected are reduced in size toward the ultimate element of the individual case, the number of units being increased in inverse proportion, the amount of travel necessary to reach all the units increases and consequently a point is reached at which the increase in travel cost offsets the gains to be had from further reduction in the size of the units. Hence sampling is often done by "clusters" of several persons or households, each cluster being selected as a single unit in the sampling. It was failure to use small units that was primarily responsible for the unrepresentativeness of the Italian sample described above.

A third principle is that the sampling may be improved if it is done by a series of steps, first by the selection of relatively large units and then by selecting a subsample of smaller units within each of the larger selected units. This can be carried further into what is called "multistage sampling." This procedure, like the use of clusters, is essentially a compromise between an even scattering of individuals and the selection of a few large concentrated groups to reduce the cost and inconvenience of travel.

A fourth principle is that sampling can be improved by utilizing information about the population available from other sources. This can be done by dividing the population into subdivisions according to such information and fixing the number of units in the sample that are to be selected from each subdivision, a procedure commonly called "stratified sampling," or in a slightly different form, "quota sampling." In the latter, for example, the number of persons to come from each city or rural area is specified, and within each area or interviewer's assignment the number of men, women, old, young, rich, middle class, poor, etc., to be obtained. Another way of utilizing information about the sample obtained from other sources is to incorporate it in the preparation of the estimates that are derived from the sample. Again, it may be used in determining the units of sampling and in other ways. The possibilities of employing available information about travel routes and the variability of important social and economic characteristics have not been exploited fully but detailed information from airplane maps and Sanford Atlases has been used to establish a Master Sample and other national samples utilized by the Bureau of Agricultural Economics and the Census Bureau. As surveys become more frequent and data accumulate in greater detail, the information available for the improvement of samples will become increasingly important.

A fifth principle is that a base can be laid for gauging the amount of variation between estimates derived from a particular sampling plan by utilizing sampling procedures that assure to each unit in the population a known positive probability of being selected. This base can be strengthened by selecting at least two units at random within each ultimate subdivision of the population.

A sixth principle is that improvement may be attained by adjusting the proportion of the sample taken from each subdivision to take account of differences in their internal variability. This principle was set forth by Neyman (15).

A seventh principle is that the size of the sample itself may be adjusted to the results attained in the successive selection of units for the sample.

This is known as sequential sampling. Its theory has been developed by Wald (23).

An eighth principle is that a combination of methods may be better than any one method alone. For example, a survey may be conducted by mail and then interviewers sent to interview a sample of the persons who did not reply. The results of the two methods are put together by appropriate weighting of each and the number of interviews can be determined to take account of their costs and value for the final results. Similarly, it may be advantageous to take a large sample by inexpensive methods and a small sample by precise but expensive methods, combining the results by appropriate techniques.

These principles have not been stated rigorously, since the inclusion of necessary conditions and qualifications would obscure their essential meaning and make this paper much too long. A thorough presentation of them will be provided in the report of the Study of Sampling which is sponsored by the Committee on the Measurement of Attitudes, Opinion, and Consumer Wants.

A PROBLEM IN THE COMPARISON OF SAMPLING METHODS

While tremendous advances have been made in sampling methods and very efficient methods have been evolved, there are a number of issues on which there is sharp disagreement among people engaged in sampling at the present time. The existence of these disagreements should not becloud the fact that the methods used by each of the parties at issue are very much better than the crude sampling methods that were employed a generation or two ago. At the risk of exaggerating its importance in the whole field of sampling, we will single out one of these issues and discuss it in some detail as an example of the questions to be investigated in studying the relative effectiveness of various sampling methods.

The issue is not very simple. Perhaps the clearest way of stating it is that there is a difference of opinion as to whether interviewers should be given any discretion in selecting the sample of persons they are to interview. The first party in the disagreement contends that it is imperative to follow a procedure in which the persons to be interviewed are selected in advance by processes of random selection or more complicated probability schemes. The second party believes that the selection of the persons to be interviewed can be left to the interviewers within rather general restrictions on the proportions of persons to be taken from each sex and from each of several broad age groups and income or economic classes. The first group claims that the latter method, which is generally known as "quota sampling," is subject to the possibility of large biases and that the degree of reliability of the results it produces cannot be estimated. They claim that their method of "area sampling" through its dependence on probability theory is unbiased and permits the estimation of the reliability of its results.

The proponents of quota sampling have not stated their point of view very systematically but in effect they claim that after the quota controls are imposed and interviewers are trained properly, their selection of respondents will be subject to relatively small biases and will approximate a random selection. They claim further that the variability due to sampling by their methods is within reasonable limits considering the approximate nature of the interviewing and measurement that is being performed and that greater attention should be paid to the interviewing problem and less concern expressed about the sampling problem.

This conflict of opinion over "area versus quota sampling" is a special case of a broader conflict over the extent to which human judgment should enter into the selection of samples (8). The advocates of area sampling believe that judgment and information should be utilized in sub-dividing the population and determining the units of sampling but that the final selection of the sample should be by some device that operates on the basis of the laws of chance rather than by a final act of human choice. The advocates of quota sampling contend that this requirement makes a sampling process unnecessarily expensive and difficult to operate. Both methods of sampling have been used in one form or another for at least fifty years but the discussion of their relative merits has been most intense during the past decade.

A fairly typical example of area sampling is to divide the entire country into regions or strata utilizing available information about their social and economic characteristics. Within each region a number of counties or similar areas are chosen by chance. Within each of these selected areas in turn a number of city blocks or small rural areas is selected by chance. Finally within each of these city blocks or small rural areas a number of households is selected by chance usually from lists especially prepared by preliminary field survey of the sample areas. These successive chance selec-

tions are so arranged that the probability of any household's being selected for the sample is the same for all households in the country. The actual procedures are usually somewhat more complicated. Essentially the method is one of drawing a sample by means of a probability device that makes use of known facts about the population (*a*) to reduce the variation that may be expected in a series of samples drawn in this manner and (*b*) to assure in the long run a close agreement between the important characteristics of the population and the average value of the estimates of these characteristics over a succession of samples. In order to carry out their field work in accordance with the sampling scheme, each interviewer operating under area sampling procedures is required to call at the households assigned to him, and, if necessary, to repeat these calls until he completes all his assignment. The rule is usually relaxed somewhat. If he is still unsuccessful after some set number of attempts, he may be excused from the necessity of making further calls. The losses from the sample must then be taken into account in the analysis of the data.

The essential feature of quota sampling is that each interviewer is requested to get a specified number of persons in each of several categories such as men, women, high income, medium income, low income, old and young, persons over forty years of age and persons between twenty and forty within the district assigned to him. The number of individuals in each category is determined from an examination of census data and other information, adjusted to take account of known changes, with the object of obtaining an aggregate sample that will match the national population in these characteristics. The underlying rationale has not been published in more than a rather general form, but essentially it rests on the assumption that if these characteristics are controlled in the selection of persons for interviewing, then other characteristics that are associated with them or correlated with them will also be controlled in varying degrees and the persons who are selected by the interviewers will represent the entire population closely on many characteristics including those of primary interest in the survey. Interviewers are also instructed to follow certain other procedures aimed at obtaining representativeness. For example, they are cautioned against neglecting the lower education groups and lower income groups that tend to be omitted from the samples by interviewers who are not so instructed. They are also instructed to scatter their interviews. While many of them are permitted to do their interviewing in stores and offices, on the streets, and in parks and other public places, they are often instructed to take most or all of their interviews in the home, and some of them in the evening in order to avoid biases due to differences in the daytime activities of different kinds of people. The quota interviewers also are aware of the fact that they are expected to get representative people and, in a variety of ways, they attempt to fulfill this purpose. Apart from these instructions and their quota requirements, the quota interviewers are free to select such individuals as they can find readily, and they are not under any compulsion to complete interviews with persons who may not be at home or may be unwilling to cooperate.

There are a number of variations of each of these methods. Moreover, the terms area and quota sampling have been used in a variety of meanings, and there are even some methods of sampling which have been designated as combinations of the two. Many survey experts refuse to accept either position but hold that one method is better under one set of circumstances and the other under other circumstances.

In studying the relative merits of area and quota sampling, we encounter a number of problems. In the first place, the question of which is the better method of sampling depends on a number of considerations. One of these, obviously, is the accuracy of results to be obtained by the use of the method. This depends on the size of the sample that is taken, and on specific details of the procedure by which the sample is drawn. It also depends upon the costs of sampling under each of the procedures in any of the varieties by which it may be applied. Unfortunately, cost data to answer this question are lacking. Some of the costs are joint costs resulting from preparatory work that is common in a number of surveys. This is especially true in area sampling. Another problem is that of the adequacy and accuracy of the estimates that are used in setting quotas. This depends to some extent on the recency or remoteness of the last census and the availability of special sets of data, such as those that arose from the registration of persons in connection with wartime rationing. There are also problems about the degree to which the quotas actually control the variables that are being studied. This depends on the relations between variables in the population. It also depends on the manner in which inter-

viewers seek out and select persons to fill their quotas. That is itself a problem of human behavior. It is hardly possible by questioning the interviewer himself to obtain an adequate and accurate description of the procedures he employs both consciously and unconsciously. There are many subtle possibilities of bias and distortion in the selection of persons to be interviewed since the interviewer is inevitably impressed by the appearance of houses and persons that he approaches, by his like or dislike of certain sections of the city and certain types of people, and by the concessions he makes to convenience in selecting the areas within which he will work. Interviewers in rural areas neglect the back roads and inaccessible places, and tend to do their interviewing on the main roads and areas close to town. Likewise they tend to miss the poorer and more disagreeable sections of a city and inaccessible persons in the upper classes who are protected by butlers or doormen or the locked entry ways to apartment buildings. These difficulties also affect area sampling but to a lesser extent.

One of the approaches that has been made to the study of area and quota sampling, as well as other types of sampling procedure, has been to search for instances in which the same population has been canvassed on the same question by different methods so that the methods can be judged from comparable trials. This approach is limited by the scarcity of surveys that are strictly comparable in the time at which they were taken, the populations that were surveyed, and the questions that were asked. It is limited also because it shows the results of two methods to be similar or different, but does not indicate which is the more accurate. Consequently, it is supplemented, when possible, by testing the results directly against independent data of established accuracy on the same questions. Tests of this type have been confined to a few factual items, such as population characteristics, and to the results of elections. In each instance, the independent data themselves are not entirely accurate, and hence some differences are to be expected as a result of their inadequacy. However, useful information has been obtained from these approaches. It shows that area and quota sampling methods both have some defects, that frequently they produce results that are quite similar, and that a more effective test of the two methods can be made only by setting up very well designed experiments on a considerable scale.

Another approach is to undertake experiments to analyze the performance of interviewers, particularly as it relates to obtaining cooperation of respondents, success in finding designated respondents at home, and the various processes by which interviewers select respondents under quota sampling procedures. For this, the initial steps have been to interview a number of interviewers and question them about the procedures that they used. Such interviews have been conducted at great length and in great detail for a few very experienced interviewers, but on the whole, they leave unanswered a number of questions about interviewer performance. Experiments have been conducted in which interviewers have been observed in the process of interviewing, sometimes without their knowing it. Further experimentation is needed.

To obtain information about the factors that affect costs and processes by which interviewers build up their quotas, an extensive analysis has been undertaken of the time that interviewers spend in each step in their interviewing. The results of this study have not yet been compiled, but they will throw light on the differences between interviewers in the rate at which they do their work and the sequence in which they fill their quota categories. They will produce data on the proportion of refusals and attempts to obtain interviews that are thwarted by the fact that no one is at home. They will also furnish some information about the time spent in traveling from one interview to another, and provide a realistic picture of the progress of an interviewer in filling his assignments. Other cost studies and time studies are being undertaken by a number of survey organizations.

Another approach to the problem of the relative merit of area and quota sampling and their variations is the study of the spatial distribution of different kinds of population regionally and within the larger cities. This has a direct bearing on the extent to which the sample must be spread out over the whole country and within cities and rural areas. It is not possible to get a complete answer to this question since the answer inevitably must differ from one variable to another that may be the subject of the study, but a substantial beginning is being made in the measurement of spatial correlation for such representative variables as education, rental as a measure of economic status, home ownership, and the vote on particular candidates and issues. We do not have very much data on the geographic distribution of at-

titudes and opinions, and particularly on their local distribution by small areas. This is a field in which the development of studies of human ecology will be helpful, but, even in this field, the results to date have been rather general and not closely applicable to the problems of sampling.

We have given considerable attention to the discussion of area and quota sampling. This issue should not be exaggerated as reflecting a deep and irreconciliable cleavage among persons engaged in sampling. The results of our studies tend to show that the differences between these methods of survey sampling are not absolute and that there are many effective variations of the methods that can be found between the two extremes. Their relative usefulness varies with the circumstances. Moreover, in the development of the fundamental theory of sampling, it appears that interviewers can be regarded as selecting devices or selectors, each of which may be represented by an operating characteristics curve with respect to the selection of persons with particular attributes in a population of a particular kind. This conception makes it possible to construct a probability model to represent quota sampling, and to that extent, provides a basis for theoretical comparisons incorporating, if they are available, specific values of the fundamental parameters of the models.

A GENERAL THEORY OF SAMPLING

The abstract analysis of sampling procedures makes it clear that they can all be reduced to a single system and that the unique characteristics that are thought to inhere in many of them are not all unique. The basis has been laid for a general and complete theory of sampling which should be of considerable value both in making it unnecessary to make certain studies that are needlessly particular and in providing a basis for improvements beyond those previously thought to be possible. It also establishes the limits beyond which improvements cannot be made in particular circumstances and provides a much more satisfactory method of measuring the efficiency of sampling methods than has been available heretofore.

The application of the general theory involves a large number of difficulties, but progress is made when these difficulties are brought to light and it becomes possible to plan and direct research toward their solutions. Moreover, the general theory of sampling fits in with a still more general theory of decision and action, in which, the role or function of information, and particularly of new information, purposely obtained to meet the needs of a problem, is determined in relation to the whole process of human behavior. This has direct connections with problems of social organization, the functioning of institutions, and the progress of science.

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BIAS IN INTERVIEWING IN STUDIES OF OPINIONS, ATTITUDES, AND CONSUMER WANTS

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(*Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations*)

ALL science is dependent on language and linguistic communication. Only through the use of language can the scientists isolate discrete experiences reflectively, group them into related systems, and derive abstract, and therefore useful, understandings of them. As a system of terms of reference, language in its more sophisticated forms has a quasi-mathematical character; its manipulation indicates possibilities of experience not immediately ascertainable from actuality and suggests means whereby these possibilities may be realized. Moreover, science is dependent upon language for the communication and independent verification of its generalizations and for their ultimate systematization in social thought and practice. This centrality of language in scientific method has long been recognized.

But when the object of scientific study is human behavior, language is central substantively as well as methodologically. For, except in purely hypothetical, extremely primitive circumstances, language is always an element in human action and always has conditioning, if not determining, effects on the course of action. Man is a perceiving creature who assesses and defines his world, intuitively or reflectively, and acts not upon that world as such but upon his representations of it. Nearly all his relationships to his world, even on the physical and physiological levels, are mediated by symbols. Language, as Sapir [1] remarked, completely interpenetrates with experience; ". . . it is so deeply rooted in the whole of human behavior that there is little in the functional side of our conscious behavior in which language does not play a part." Human behavior cannot be understood, therefore, by attending to the patterns of man's actions that are open to non-communicative observation, although attention to these externals is required. Understanding will result only from penetrative procedures which reveal the modes of symbolization and which exhibit the relation of the symbolic to the other phases of personal and group action. The observation, description, and interpretation of linguistic behavior is an aspect of nearly every problem in the psychological and social sciences.

Moreover, in the scientific study of human behavior, language has a kind of methodological use that it does not have in the other sciences. Language provides a means whereby the scientist gets access to the phenomena in which he is interested. Just because language is an integral part of the phenomena, the techniques and procedures employed in isolating these phenomena and determining their character and interrelations must frequently be linguistic. In a way not true in other areas of research, the study of human behavior requires cooperation between the student and the studied. Under appropriate symbolic guidance or provocation by the student, the one whose behavior is being studied must make manifest the covert aspects of behavior by reconstructing his actions symbolically. In the study of human behavior, therefore, critical attention must be given to language, not only as a pervasive element in the phenomena being explored and as a medium for precise, systematic formulation of the accumulating fund of knowledge, but also as an instrument used in exploring the phenomena.

This instrumental use of language is, of course, a common aspect of all human relations. In order to get a better basis for our adjustments to other people, all of us—as workers, parents, friends, citizens, etc.—try to find out what others think and how they feel about things—the boss, a labor union controversy, surrealism, relations with Russia, a Negro invasion of our residential neighborhood, the cost of living, a family problem, President Truman, the sex life of the human male, or anything else that we find interesting or problematical. We inquire into others' intentions and what they propose to do about them so that we may know better how to guide our own actions. The scientific use of this approach is, therefore, only an extension and refinement of folk practice

[1] Sapir, Edward, Language, *Ency. of the Soc. Sci.* **9**: 157, 159, 1933.

for the purpose of securing results of greater verity, relevance, and generic significance.

There are many ways in which language has been used by psychologists, sociologists, and other social scientists as a means of access to the phenomena they are studying. For historians especially, but to a certain extent for other students of human behavior, both individual and social, documentary materials have long been used as sources of factual data. Such materials range from diaries, letters, life histories, and other intimate, personal documents to the impersonal content of mass media of communication. In dealing with certain types of problem, the qualitative and quantitative analysis of such materials has proved highly useful.[2] But generally the approach is of limited or incidental utility because it truncates the act of communication by isolating the linguistic elements from the rest of the behavioral context in which it occurred, except as these uncaptured elements may be inferred, and because it is usually applicable only to acts which have already terminated so that experimental intervention for the purpose of checking hypotheses is not possible.

Considerable use has also been made of language behavior as it naturally occurs in uncontrolled life situations as a means of getting insight into the causes of different forms of behavior. This is a type of participant-observation which is exemplified by a large body of social anthropological studies and by many of the investigations of political parties and other types of collective behavior. It has also been used, in modified form, in connection with certain types of group experimental work. In the latter cases, "life situations" that are assumed to be like those that arise naturally are experimentally produced, but the language behavior that appears within these situations is left free and spontaneous and is recorded for subsequent analysis. For the study of specific forms of action rather than basic general attitudes, this method of using language is also subject to the limitation that it precludes direct and immediate experimental intervention as the language behavior develops.

Most important among the ways in which language is used methodologically by students of human behavior is careful, systematic interviewing, in which respondents are induced to aid in the exploration of their own actions. The use of the interview in scientific research ranges from the incidental guided reporting on the covert aspects of a subject's responses in simple laboratory experiments to the non-directive depth interviewing of the psychiatrist. It is an approach which holds out the greatest promise of scientific usefulness because it is not subject to the limitations that are inherent in other uses of language. It has the essential advantage of flexibility; by its use the student is able to initiate the behavior he wishes to study, to intervene in it, and to manipulate it experimentally in terms of the purposes guiding his study.

Perhaps in no other area of research is the degree of dependence upon the interview in isolation from complementary, concurrent forms of observation quite as great as it is in the study of opinion, where the aim is generally to find out, not how and why people acted as they did in past situations, but how they are likely to behave in situations or types of situations that may arise in the future. The student of opinion derives most of his data by means of interviewing, and usually by interviewing a large number of people who vary widely in their vocabularies and in other significant respects. The problem of securing data that are homogeneous and free from error is both important and difficult. At every stage in the development of an opinion survey, of course, there are many ways in which errors are likely to be introduced into the findings unless adequate precautionary measures are known and utilized. But errors arising from the handling of the interview situation are crucial, because it is in the interview that data are elicited and recorded. All subsequent analysis and inference, no matter how ingeniously contrived, will reflect, in some degree, these original errors. That there are substantial response errors and that responses are easily and frequently biased have been demonstrated by many analyses of interviewers' reports.[3]

No sophisticated analysis is necessary to convince this audience that inferences about the opin-

[2] Thomas, William I., and Florian Znaniecki, *The Polish peasant in Europe and America*, 5 v., Boston, Richard C. Badger, 1918–1920. This was one of the first research projects to make systematic use of personal documents.

See also: Blumer, Herbert, An appraisal of Thomas and Znaniecki, *The Polish peasant in Europe and America, Social Science Research Council Bull.* 44, 1939.

Berelson, Bernard, and Paul F. Lazarsfeld, The analysis of comunications content: preliminary draft, March, 1948. Mimeographed. This is the most recent, systematic statement on content analysis.

[3] McNemar, Quinn, Opinion-attitude measurement, *Psychol. Bull.* **43**: 289–374, 1946.

ions of other people based on the results of interrogation are frequently misleading. Everyone has testimony from his own efforts to understand his children, friends, colleagues, or constituents. We know that indifference, evasion, concealment, dissimulation, misunderstanding, and self-ignorance often color the answers others give us. At times we do not know what we ourselves think, how we feel, or why we think and feel as we do about some matter. Or if we do know, or think we know, we catch ourselves trimming our self-descriptions, wittingly or unwittingly, to fit the occasion; we want to conform, to please, to shock, to appear intelligent, or otherwise to create the "right" impression. At other times, knowing that we have never given a thought to the subject before and really have no opinion about it, but feeling that everyone should have an opinion, we trump one up on the spur of the moment in answer to a question that is put to us. Facts of this sort are well known.

But we do not know sufficiently well how to detect with certainty sources of error in the interview and how to reduce or eliminate them. To acquire operational knowledge of this sort will require painstaking research and experimentation. We have learned much since Stuart Rice published his little essay on "Contagious Bias in the Interview,"[4] but we have much more to learn. Notwithstanding the fact that we are now able to get fairly precise and dependable indices of the general state of opinion at a given time in a given population, our procedures for setting up and conducting interviews still do not enable us to determine within tolerable limits of assurance what qualitative and quantitative differences there are in the opinions of individuals. Unless such differences among individuals are known to be valid and reliable, any correlations that are found between a given opinion or degree of opinion and other opinions or other characteristics of individuals are untrustworthy.

The first step in any effort to reduce or eliminate error and bias in interview results is to examine the whole interview situation in order that techniques and procedures may be adapted to known opportunities for bias. The examination would have to cover not only the variations in replies that may be induced by changes in the questions asked and in the mechanics of questioning but also the variations arising from the relation between the interviewer and the respondent under the varied circumstances attendant upon the interview itself. A detailed examination of the interview situation is not possible here, but three of its characteristics may be briefly stated in order to suggest the scope and difficulty of the problem of deriving from the interview valid and reliable results: (1) its artifactual character, (2) the variability of respondents, and (3) the variability of interviewers and of interviewer-respondent interaction.

(1) Every interview situation is an artifact, created for the purposes of a particular study. The interviewer is typically a stranger to the respondent; his visit interrupts the respondent's customary routine; his auspices and purposes are not initially known by the respondent; and the entire experience of submitting to questioning for the impersonal reasons given the respondent is a new and somewhat embarrassing one for him. The interviewer may be suspected of ulterior motives—of being an F.B.I. agent, a magazine or book salesman, a promoter of some cause—and, even when he is accepted as just a poll-taker or a disinterested student of public opinion, he stands as some kind of representative of the outside world capable of passing judgment on the respondent's ignorances, prejudices, private opinions, and unconventional attitudes when and if he succeeds in ferreting these out. On the other side of the relationship, the interviewer finds each new respondent and each new interview situation so different from the preceding ones as to require a high degree of insight and skill in adjusting himself to it. He must, in a sense, assimilate the point of view and frame of reference of each new respondent before any effective communication can take place. Under such circumstances it is extremely difficult for the interviewer to establish with the respondent a condition of rapport—such a relation as will facilitate honest, frank, pertinent responses to the questions asked. Yet rapport must be established if the data derived are to be valid indicators of attitude, opinions, and behavior.

(2) Respondents vary widely in interest, comprehension level, degree of information, and many other characteristics which affect the attention they will give to, and the meaning they will get from, questions that are put to them. They attach different meanings to words and interpret either statements or questions differently because of differences in their frames of reference. We know that their answers can be made to vary with

[4] Rice, Stuart A., Contagious bias in the interview, *Amer. Jour. Sociol.* **35**: 420–423, 1929.

different types of question wording, a different order of questions within a questionnaire, the degree to which questions are personalized, the form of the question (yes-no, multiple choice, open-ended, etc.), the provision that is made for "don't know" and "it depends" answers, and a variety of other differences in the way questions are formulated and presented. Precise determination of the stimuli that are presented to respondents by way of interview questions is difficult because of these variations in respondent characteristics. Hence it is hard to ascertain the precise meaning of responses that are given by different respondents even to questions that are presented in identical ways by the same interviewer. Substantial amounts of error may appear in the over-all results unless these variable meanings are ascertained. To some extent, the range of possible interpretations of interview questions can be limited by careful construction and pretesting of the questionnaire. The questionnaire is really a part of the interview situation, and standardizing the questionnaire on the basis of exploratory interviewing is merely one way of assuring that, within limits, the stimuli that are being brought to bear in the interview are the same for all respondents and are relevant to the objectives of the study. But standardization of the questionnaire is not sufficient for this purpose; access to and control over the variable meanings respondents will assign to these stimuli can be had only in the handling of the actual interview.

(3) The actual interviewing has to be done by a corps of interviewers, each of whom is a person varying in his appearance, manner, affiliations, attitudes, etc. Each interviewer imports into the interview situation a variable set of factors that may affect each respondent in diverse ways. His characteristics, real or imputed, as they appear to the respondent and his reactions throughout the interview are of first importance among the influences that may seriously bias answers to questions. The respondent's immediate perceptual reaction to the interviewer may, for example, bring into play prejudicial or prestige factors that make him reticent, captious, or cooperative, and the interaction that develops between respondent and interviewer, much of it on the non-symbolic level, may affect in many ways the verity and completeness of responses. At the worst, perhaps, the respondent may infer the interviewer's own attitudes and opinions and be influenced by them. Thus interviewer variability, like respondent variability, makes it difficult even with a perfectly designed questionnaire to exclude from the interview situation all types of stimuli other than those to which reactions are solicited. But it is not only the interviewer's own characteristics that complicate the situation, for reliance has to be placed on the interviewer in the effort to detect and control other sources of error and biasing factors in the situation. The responsibility that falls upon his shoulders in any opinion study utilizing interviewing is heavy, for in considerable degree he determines whether interviews will realize their aim of discovering how individuals or groups respond just to those values or problems with which the study is concerned. The isolation, measurement, and control of interviewer effect in attitude and opinion studies is, therefore, a technical problem of major importance.

A good deal of attention has already been given to this problem. Surveying agencies have accumulated many useful insights from observations of their own operations and analysis of their results. A few special studies have subjected interviewer bias to empirical investigation, but these are admitted by the researchers themselves to be limited in scope and somewhat inconclusive, except that in every instance they have found evidence of bias. The conclusions that have been reported are chiefly these:

(1) The attitudes and opinions elicited or reported by interviewers have been found to be positively correlated with the interviewers' own attitudes and opinions (Cantril and others).[5]

(2) An inverse relationship has been found between the degree in which interviewers' and respondents' opinions coincide and the size of town in which interviewing occurs (Salstrom).[5]
But it has been suggested that this relationship is affected by the stranger-acquaintance relation between interviewers and respondent, since familiar interviewers may act "symbolically" to make salient the conformity agents within the small community (Hyman).[6]

[5] Cantril, Hadley, and Research Associates, *Gauging public opinion*, 318 + xiv, Princeton Univ. Press, 1944.

[6] National Opinion Research Center, Report of progress on the isolation, measurement, and control of Interviewer effect to Committee on Measurement of Opinion, Attitude, and Consumer Wants of the National Research Council and the Social Science Research Council, November 15, 1947.

(3) Discrepancies and similarities between interviewers and respondents—class, race, etc.—have been found to hamper rapport and create a distortion of true opinion (Katz and others).[5]

(4) High interviewer affect on an issue—strong feelings about it one way or the other—biases the results of interviews, especially when the interviewer is dealing with respondents whose own attitudes are uncrystallized (Cahalan).[6] However, in some instances, the correlation between interviewer's and respondent's replies appears to be highest for questions on which interviewers have no opinion (Hyman).[6]

(5) Interviewer bias gets into recorded responses with greater effect when these responses as actually given were vague, ambiguous, tentative, qualified since the interviewer, through projection, tends to classify them in the direction of his own bias (Connelly).[6]

(6) Interviewers' expectations as to types of respondent—their beliefs about the characteristics of uneducated people, poor people, housewives, or Negroes, for example—produce distortions in survey data (Hyman).[6]

(7) "Highly experienced" interviewers have been found to introduce as much bias into their reports as less experienced ones (Cantril and assistants) [5] although the criteria of experience used in these demonstrations were extremely crude (McNemar).[3]

(8) The degree of interviewer bias has been found to vary with different types of question construction (Cahalan, Tamulonis, Verner) as with open questions as opposed to the precoded type.[7]

(9) By and large, interviewer biases in one direction tend to cancel those in the other direction, although bias due to lack of rapport does not cancel out (Cantril and associates).[5]

(10) Interviewer prejudices and predilections, often operating intuitively, affect selections of respondents to fill quotas and thus impair the representativeness of the findings from surveys (Stephan).[8]

The foregoing list by no means includes all the hypotheses and hunches arising from reflection,[9] nor is it even a complete review of all the tentative conclusions arising from empirical research. But it is sufficiently representative to indicate that at least crude beginnings have been made in attacking the problem of the operation of bias in interviewing. What is now needed is to refine present hypotheses and to supplement them so that every probable biasing factor is covered, not only as to its reality and the magnitude of its effect, but as to its modes of operation. It will not be enough to know what factors associated with interviewers and interviewing do bias data and in what degree. It will be necessary to know further how and why these factors exert their biasing effects and what conditions are requisite to their management.

With the hope of making some contribution toward meeting these needs, the National Opinion Research Center has recently undertaken a two-year program of studies of bias in interviewing as part of the research program sponsored by the Committee on Measurement of Opinion, Attitudes, and Consumer Wants of the National Research Council and the Social Science Research Council. The studies are made possible by a grant from the Rockefeller Foundation. They are under the direction of Herbert Hyman, with the counsel and cooperation of Frederick Stephan, Paul Lazarsfeld, Rensis Likert, and other competent advisers.

The first and current stage of the research is the construction of a complement of hypotheses having the character and scope prescribed above. This is being done (*a*) by careful scrutiny and analysis of the interview situation reflectively and through a variety of empirical approaches, including clinical interviewing of experienced interviewers and analysts; and (*b*) by careful review and some preliminary analysis of the data that have accumulated in the files of various research agencies and individuals. As hunches are devel-

[7] Cahalan, Don, Valerie Tamulonis, and Helen W. Verner, Interviewer bias involved in certain types of opinion survey questions, *Int. Jour. Opinion and Attitude Res.* **1**: 63–77, 1947.

[8] Stephan, Frederic F., Progress report on the study of sampling to the Committee on Measurement of Opinion, Attitude and Consumer Wants of the National Research Council and the Social Science Research Council, November 12, 1947.

[9] Connelly, Gordon M., and Natalie Harris, A symposium on interviewing problems, to be published in a forthcoming issue of the *Int. Jour. of Op. and Att. Res.* **2**, 1948.

oped, they are being tested in a preliminary way by systematic analysis of existing data when relevant data are available, and/or by quasi-experimental projects that are carried out in connection with surveys undertaken for other purposes. The results of this first phase of the research will clarify the specific problems and will provide a theoretical framework within which these problems may subsequently be studied in more conclusive ways.

The second part of the program will be experimental. As now tentatively projected, it will involve (*a*) a specially designed field survey comprising a series of interviews with address-controlled matched samples, conducted by a few specially selected and trained interviewers in three or four selected communities, the interviews being administered under as close observation as is possible without interfering with the normal interview situation; (*b*) a supplementary series of interviews under carefully controlled conditions by the same interviewers, using the same questionnaire, but with complete oral and visual recording of the interviewer's and respondent's behavior.

Such an experimental set-up will, it is hoped, bring under observation most of the variables that are strongly suspected of having a significant biasing effect upon the replies elicited and recorded by interviewers. Factors inhering in the social or cultural setting may be partially controlled through selection of the sample communities. Interviewers may be selected in terms of the characteristics that are thought to be biasing. Different types of subject matter, different question forms, different methods and degrees of probing, and different methods of determining the definiteness, strength, and bases of attitudes and opinions may be admitted or kept out of the questionnaire as the hypotheses to be tested require. Similarly, predesignation of respondents and prescribed methods of interviewing for which interviewers will be specially trained will bring other supposedly relevant factors under control. Recording of interviews will, on the other hand, make it possible to observe the interview situation in detail with greater reliability.

What will be attempted in this second or experimental part of the study is *not* one big experiment that will solve once and for all the whole problem of bias in interviewing. The controlled survey and the recorded interviews will not be *an* experiment. Instead they will provide a general framework of controls within which various specific problems may be studied experimentally, each of which will be precisely defined and will require a particular selection and combination of empirical factors. But opportunity will be provided also for further qualitative exploration of the dynamics of bias in the total interview situation and for experimentation with preventive and corrective measures.

While the results of this program of studies will certainly not be conclusive, they should at least make possible substantial improvements in our survey procedures. They should enable us to do a somewhat better job of selecting and training interviewers, designing questionnaires, and adapting interviewing methods to known probabilities of bias. It is hoped, however, that the studies will go beyond methodology in this larger sense by sharpening and deepening our insights into human conduct as it is related to and affected by communication. They should, for example, enlarge in some degree our knowledge of the human motives underlying types of action, of how prejudices and antipathies affect the run of attention, modes of interpretation, and value judgments, how hierarchies of attitudes (or values) are related to individual and group behavior, and the role of symbolic representation of reality in conditioning or determining actions. It is only as we are able through these studies to sharpen and deepen our insights into such substantive matters that we will make any significant contribution, methodologically or otherwise.

THE USE OF PANELS IN SOCIAL RESEARCH

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(*Read February 7, 1948 in the Symposium on Research Frontiers in Human Relations*)

THE following remarks are designed to draw attention to a fairly recent development in social research. In its bare essentials, the type of study to be discussed consists of repeated interviews made with the same group of persons. The people participating as subjects in such studies are commonly known as panel members and the whole procedure has become widely known under the name of panel technique.

There are two main types of research problems to which the panel technique is likely to be applied. If the effect of some specific event or series of events is to be studied, then we have the first type of situation in which the panel technique may be used. In one such case, a sample of voters in an Ohio county was kept under observation for six months during the 1940 Presidential campaign, the purpose being to study what effect the propaganda of the two parties had upon the way people made up their minds.[1] In another case, the American Association for the United Nations wanted to find out the best way of getting Americans more interested in the progress of U.N. activities. A sample of persons in a mid-West city of about 800,000 was interviewed about their attitudes towards the United Nations and the actions of the United States in foreign affairs. An intensive informational campaign was conducted by this organization and after the campaign was over the same sample was interviewed again.[2] In a similar way, advertising agencies sometimes use panels to study the effectiveness of their promotional efforts.[3]

The other main type of panel study is somewhat more difficult to describe because no major findings are yet available in the literature. In a society as complex and changing as our own, the individual is continually placed in a situation where he must reconcile the different and variant elements of his experience. A Quaker who is a convinced pacifist sees the country endangered by an enemy. How will he resolve the conflict between his pacifism and his patriotism? A convinced Communist sees the Soviet Union making moves which he considers imperialistic. How will he reconcile his party loyalty and his intellectual judgment on a specific political issue? But we don't need to remain in the area of big issues to look for problems of this kind. In everyday life almost everyone is continuously under cross-pressures of some kind. People belong to different social groups which may have conflicting interests. The individual must make all sorts of choices among his needs, desires, and situational demands, some of which are relatively important, others relatively insignificant.

The study of people under cross pressures is one of the major concerns of social science today. In going through recent social science literature one often comes across statements of the following sort, "In getting higher education the English Catholic must choose between ethnic affiliation and religion; he generally chooses to study with his Protestant ethnic fellows at McGill University. . . ."[4] The application of the panel technique to problems of this sort allows a greater degree of analytical precision. It would allow us to state, for example, the proportion of English Catholics who go to McGill for their higher education and the proportion who go to Catholic institutions, and to compare intensively those who resolve the conflict between their ethnic affiliation and their religion in one way with those who resolve this conflict in another.

The understanding of what actually transpires in such situations will make for tremendous gains in the understanding of social change. The application of the panel technique to this area of social science interest will be one of its major contributions. By keeping sets of people under repeated observation, we can register the changes

[1] Lazarsfeld, P. F., B. Berelson, and H. Gaudet, *The people's choice*, N. Y., Duell, Sloan and Pearce, 1944.

[2] National Opinion Research Center, Report No. 37a, Cincinnati Looks Again.

[3] Root, A. R., and A. C. Welch, The continuing consumer study: a basic method for the engineering of advertising, *Jour. Marketing* 7, July 1942.

[4] Hughes, E. C. *French Canada in transition*, 86, Univ. of Chicago Press, 1943.

they make in their attitudes, affiliations, habits, and expectations. We can learn which of the various attitudes, affiliations, etc., are more basic and hence more constant and which are more superficial and changeable. We hope to determine, if elements change, which element in a psychological or social situation is the more dominant one controlling the changes in the other factors.

The outstanding example of such a study is that undertaken by Theodore Newcomb of the students of a "progressive" college attended by the daughters of well-to-do families. The faculty of this college was quite liberal but the background of the girls quite conservative. For four years the investigators observed the various ways in which one group of girls resolved this conflict.[5]

The reader who is somewhat acquainted with social science literature will at this point raise a justified question especially with reference to the first type of study. If we want to know the effect of a political campaign or a similar event, why do we have to reinterview the same people? Couldn't we interview one group of respondents before the event and a similar one after the event. By comparing the two, the argument runs, we would get a fairly good idea as to the influence which the event had. Numerous examples of this kind come to mind. Many of us have seen public opinion polls taken, for instance, before and after the President made a major public announcement. If people think better of him after the speech then we are sure the speech was a success. Poll data are available which show that the attitude of the average American to the Russians improved every time they were victorious in a battle during the war and slumped every time the Russians, after the war, made a move against one of their neighboring countries. This type of study is undoubtedly of very great value and is usually called a trend study.[6]

It is important to consider the differences between such trend studies and the panel technique. A considerable amount of additional information is obtained by reinterviewing the same people. The most important difference is our ability to single out in a panel study exactly who are the people who change. Once singled out, the changers can be subjected to more intensive study to determine the psychological and social-psychological elements which operated to produce the changes in question. A trend study may show us the net impact of events on opinion. A panel study can allow us to single out the individuals who changed their opinion in the course of the repeated interviewing, to probe for the psychological meaning of the event, and the role played by the various mass media of communication in the change. By interviewing the same people at least twice, we can answer questions such as the following: Are people more likely to change when they are very interested in an event and follow it in great detail; or when they are only slightly concerned and know of it only in a casual way? Some preliminary evidence seems to show that the latter is more likely to be the case. There are many proverbs which claim that men are more apt to shift than women and many others which claim the exact opposite. The panel technique permits us to say whether men or women are more likely to shift their opinions. Incidentally, the results so far do not seem to point to any sex differences.

The study of actual changes often leads to unexpected results. At the time that Senator Black was appointed a judge of the Supreme Court, he was accused of having been at one time a member of the Klu Klux Klan. It happens that there is some information available on who was affected by this allegation which suddenly threatened to change the image of a liberal into that of a reactionary. Although Senator Black received about the same amount of approval before and after the allegation, a kind of game of musical chairs took place. Jews and Catholics turned against him while about an equivalent number of Protestants were more in favor of his appointment than before the storm broke.[7]

The last example points to a second value of the panel technique. Trend studies often indicate that an event has not brought about any net change in opinion. But it might very well be that underneath this apparent constancy, there is a great amount of shifting of positions which can only be found out if the same people and their attitudes are traced over a period of time. At the beginning of the present 1948 presidential campaign, there is some indication of a new development in American politics. As long as Roosevelt was alive, there was a strong feeling in the population that the Democratic Party was the party of the common

[5] Newcomb, T., *Personality and social change*, N. Y., Dryden Press, 1942.

[6] Bruner, Jerome S., *Mandate from the people*, N. Y., Duell, Sloan and Pearce, 1944; Cantril, Hadley, *Gauging public opinion*, Princeton Univ. Press, 1944.

[7] Lazarsfeld, P. F., The change of opinion during a political discussion. *Jour. Ap. Psychol.* **23**: 131–147, 1939.

man whereas the Republicans represented more the interests of the wealthier sections of the population. There are indications that this appraisal of the two parties has changed somewhat and that voters, especially among the working class, are less sure than before which of the two parties represents their interests better.

Suppose that one further development takes place (for which there is no evidence but which we bring in to make our example more dramatic); some sections of the business community might feel that their interest in an active recovery program in Europe is better served by a Democratic administration. Then we might have at this moment an internal shift in the social stratification of the two parties which might go beyond any net change in both which polls or the election might show up. Such a social restratification of the major parties has taken place several times in the political history of this country. The historian looking back over this period many decades hence will not miss such a development. But if we want to know and understand it at the time it happens, we have to make studies of repeated interviews with the same people.

This is not the place to go further into detail on the comparison of panel and trend studies.[8] We shall turn rather to the other type of panel study in order to show briefly some of its considerably more complex technical aspects. The following table (table 1) exemplifies some of the technical difficulties. It is taken from a small group of people who were interviewed twice during a presidential election. Each respondent was asked two questions: How he intended to vote and whether he felt that the Republican candidate if elected would make a good President. Because both questions were each answered on two different occasions by each respondent we have four pieces of information about each member of the panel. Table 1 classifies these replies first according to whether they were obtained at the first interview or at the second. For each interview we can then sub-classify the respondents into four groups: those who wanted to vote Democratic and who were also personally opposed to the Republican candidate; those who wanted to vote Democratic but personally respected the opposing candidate; those with Republican vote intentions who, however, disapproved of their party's candidate; and, those with Republican vote intentions who also approved of the candidate.

All the information which can be obtained from two questions and two interviews with the same respondents can be represented in the following type of table.

Let us first look at the last column. Most Democrats are against the person of the Republican candidate and most Republicans are for him. But 59 of the 266 respondents have a kind of personal attachment. Twenty-four Democrats think that the opposing candidate is all right while 35 Republicans, although they intend to vote for their party, obviously wish that another candidate had been put up.

Now let us look at the bottom row of figures which come from the second interview. The number of people with such detached views has decreased. Obviously, what the campaign has done is to intensify partisan feeling. Only 15 Democrats now have a good word to say about the Republican candidate and only 27 Republicans have any doubts left about him.

TABLE 1

First Interview	Second Interview				
	Dem. Ag.	Dem. For	Rep. Ag.	Rep. For	Total
Dem. Against	68	2	1	1	72
Dem. For	11	12	0	1	24
Rep. Against	1	0	23	11	35
Rep. For	2	1	3	129	135
Total	82	15	27	142	266

But that is not all that we would like to know from this table. How do people reconcile their vote intention and their opinion on a specific issue? Do the Democrats who like the opposing candidate shift to him or do they remain Democrats and start to see him in a darker light? The answer is given in the second row of our table. There is only one case of the former, but 11 cases of the latter type. And it so happens that similar figures prevail for the Republicans. Let us look at the third row where we find the respondents who at the first interview intended to vote Republican but didn't like their candidate. One of them switched to the Democrats but 11 now feel better towards their candidate. In this one case there is no doubt that most people adjust their cross-pressures in a one-sided direction. If their party loyalties are in conflict with a specific opinion of their own they

[8] Interested readers will find such a discussion and concrete examples in chapter 10, The panel, in *Say it with figures*, H. Zeisel, N. Y., Harpers, 1946.

are rather more likely to maintain their party loyalties and change their opinion.

This is of course just one example from which no general conclusion should be drawn. But it shows the type of problem and the type of procedures which derive from the use of the panel technique. Just for the record it might be mentioned that the statistical analysis of tables like the preceding one is quite difficult and proper procedures are still in the process of development. It can easily be seen how many more problems would arise if we had more than two interviews and more than two questionnaire items to deal with.

Besides the difficulties in analysis discussed above, there is one other drawback of the panel technique. There is a danger that we may change our respondent's attitude by the very fact that we reinterview them repeatedly. In some cases the danger is obvious. Suppose, for example, we interview people during a vaccination campaign. If we repeatedly ask people whether they have been vaccinated, our inteviewers will probably act as reminders and speed up the success of the campaign in our panel beyond the performance of the population at large. In this case, then, the results of our study will be quite misleading. It could of course happen that our interviewers antagonize the respondents and as a result they might be less likely to get vaccinated. In other cases the panel bias is not likely to be marked. If interest in an election is high and everyone talks about it, the fact that a respondent has been asked about his vote intentions is not going to influence him very much. In any case this is a matter for concrete study. We cannot tell in advance when bias is likely to exist or not.

Actually, a few such studies of bias have been made. The technique used is fairly simple. At the time the panel is picked out a second group of respondents known as a "control group" is set up as closely matched to the panel as possible. This second group, however, is not interviewed until the whole panel study nears its end. At the time the last interview is made with the panel, the control group is also interviewed. From a statistical point of view the two groups were originally alike and should therefore at the end of the study show the same distribution of attitudes were it not that the panel group was interviewed repeatedly. Whatever significant differences show up between the two groups can be attributed to the effect of the panel bias.

Two examples should give an idea of how much work there is still to be done in this direction. During a presidential campaign it was found that the distribution of opinions in the panel was no different than in the control group. But the panel made up its mind somewhat quicker. Under the impact of the repeated interviews the "Don't Knows" in the end were less numerous in the panel than in the control group. This is a very encouraging result. On the other hand it was found that if people were repeatedly interviewed about their newspaper reading habits the panel group was likely to do more newspaper reading than the control group. The reappearance of the interviewer obviously stimulated the reading interests of the panel members. There was some indication, however, that approximately from the third interview on this effect became less and less marked. It might very well be that if the panel had gone on longer, the panel bias would have disappeared in the end.

TABLE 2

		2ND INTERVIEW Yes	No	Totals
1ST INTERVIEW	Yes	50	50	100
	No	50	850	900
Totals—		100	900	1000

TABLE 3

		2ND INTERVIEW Yes	No	Totals
1ST INTERVIEW	Yes	400	100	500
	No	100	400	500
Totals—		500	500	1000

There are many operational problems involved in panel studies just as in any other large-scale research operation. How can we get people to participate in a panel and to stick to it? How do we substitute for unavoidable losses? Is it sometimes possible to correspond with panel members by mail rather than to make personal contacts? Should we handle a panel as the American Senate is handled, always substituting part of it by new members?

Finally, there are a number of serious statistical problems to be dealt with. They all center around the concept of turnover. The following two tables exemplify the problem. They each represent one question on which people have been interviewed twice.

In the first question (table 2) 100 people changed their minds one way or another. On the second question (table 3) 200 people did so. One might feel that the turnover on the second question is therefore greater. But one must consider that many fewer people said "Yes" to the first question at the time of the first interview.

One therefore cannot expect as many people to change as in the second case. It might be more advisable to compute the turnover as percentage of the people who said "Yes" both times. This would give a turnover of 200 per cent for the first and 50 per cent for the second table and now we would have to say that the first question has the larger turnover. There are obviously still many other ways in which turnover can be described. What index we can use to describe best the turnover in such tables is a very vexing problem, especially because most all of the statistical treatment of panel data goes back to this one point. But this is not the place to deal with such technical matters at length. It is preferable to end up with some more general theoretical considerations which will show the place panel studies are likely to hold in the social sciences in the coming years.

Basically, what we do in a panel study is relate information obtained at one time to information obtained at a subsequent time. We are in the center of what has come to be called dynamic social research. We study changes and we want to explain these changes. We know who changed and we have information on people prior to their change. Explaining the change necessarily means to relate this previous information to the subsequent change. Everything will depend therefore upon how ingenious we are in deciding what information we should gather at different time periods. To exemplify the problem more clearly, let us assume that we are dealing with a panel of people who are about to move into a public housing project where Negroes and white will live together.[9] If we center our attention on the whites then we know in advance that some of them will get along with their Negro fellow tenants and some will not. Some will improve their ability to get along with people of other races and some will not. What information should we collect from all these prospective tenants prior to the time they move into the housing project to help us explain what shifts in racial attitudes will take place?

We will obviously want to know their race attitudes prior to their entrance into the housing project. But it will also be important to know their *expectations*. It may turn out that the greater their initial uneasiness the more will they be pleasantly surprised by reality. On the other hand we know that some people have a hard time experiencing "reality," and if they enter a situation with apprehension they behave nervously and start trouble. Some sort of index of psychological flexibility is needed.

Pieces of information about the psychological predisposition of the respondents have been called *intervening variables* because they intervene, as it were, between the individual's reaction and the situation in which he is placed.[10] In the example given above, where a group of individuals are about to enter a public housing project, we have people who will be subject to the same external experience. They will, however, react differently. Between the external situation and the individual response there intervene certain psychological and social characteristics which channel the response in an individually characteristic fashion. Social psychologists in recent years have developed out of their experience many hypotheses as to which intervening variables are of importance. We talk of a person's *level of aspiration* or of a person's *expectations,* indicating that we consider that such information will be of value in interpreting how the individuals will react to the situations in which they are placed.

The important intervening variables have to be ascertained before expected changes take place. To follow through with the example given above, we should know as much as we can about the panel members before they move into the inter-racial housing project. Once they have been living there it is too late to look for such information, for we can never know then whether what we have found has not already been influencd by their new experience. This is, of course, exactly where the importance of the panel technique lies. We periodically study people's attitudes, expectations, and aspirations. We find out what has happened to them between interviews: what they read, with whom they talked, what external events impressed them, etc. Both the situational factors and the intervening variables change continuously. Our analysis would weave back and forth from these two series of data, expressing, in one case, reaction to the situation as a function of some psychological predisposition, and, in another, the psychological predisposition as a function of the changing situation. We would want to know how

[9] An especially rich source for such explanatory variables will be found in a housing study organized by the Lavanburg Foundation under the direction of Robert K. Merton.

[10] The interested reader will find a thoroughgoing discussion of important intervening variables in Sherif, M., *An outline of social psychology,* N. Y., Harpers, 1948.

people's expectations affect the way they react to changes in their environment; and how the environment experienced changes their hopes and concerns.

On more than one occasion it has been said that one of the difficulties which impede the progress of social science is the fact that we cannot experiment with human beings in the same way that the agricultural station experiments with animals and plants. It should not be overlooked, however, that life itself is in a very real sense a continuous series of experiments. In the course of time, almost everything conceivable and sometimes things previously inconceivable happen to one group of persons or another. Although many of these events are, as yet, unpredictable, some events, fortunately for our purpose, occur with sufficient regularity or frequency so that if we know just what sort of persons will be subject to them, we can observe the various ways in which they will respond. Panel studies are conducted, usually, on the impact of events of a given predictable regularity such as voting in a presidential election, exposure to certain advertising, etc. If we find the right statistical technique we will be able to interrelate "stimulus, predisposition, and response" and with time and experience our hope is to understand, predict, and control human behavior more successfully.

The panel technique discussed here is an expensive and rather slow research operation. A social science, unfortunately, will not develop overnight, and, if we want to develop one, we will have to pay for it in time and money. We cannot prefer mere speculation because it comes to us quickly or simpler methods like cross-sectional polls and artificial laboratory experiments because they are cheap. The panel technique is just in its beginnings—we have just begun to explore some of the implications of its use—and most of its future development will require long and arduous work. Among the many lines along which the methods of the social sciences are developing, the panel technique seems to be one of the most promising for the future of a fuller understanding of human behavior.